THURSDAY, JULY 27, 1871

MR. CROOKES ON THE "PSYCHIC" FORCE

WITH a boldness and honesty which deserve the greatest respect, Mr. Crookes has come forward as an investigator of those mysterious phenomena which have now been so long before the public that it is unnecessary to name them, more especially as their generally received name is very objectionable.

Two things have contributed to retard our knowledge of these strange events. In the first place, until lately few men of name have been associated with their occurrence, so that outsiders have not had the facts put before them in a proper manner. In the next place, we are inclined to endorse the remark of Mr. Crookes, that men of science have shown too great a disinclination to investigate the existence and nature of these alleged facts, even when their occurrence had been asserted by competent and credible witnesses.

Before adverting to the results obtained by Mr. Crookes, a few words may be said about our mode of procedure in accepting testimony.

Let us suppose that a man comes before us as a witness of some strange and unprecedented occurrence. Here it is evident that we are not entitled to reject his testimony on the ground that we cannot explain what he has seen in accordance with our preconceived views of the universe, even although these views are the result of a long experience; for by this means we should never arrive at anything new. Our first question is manifestly one regarding the man's moral character. Is he an honest and trustworthy man, or is he trying to deceive us?

Let us assume that we have convinced ourselves of his honesty; we are then bound to believe that *he thought he saw* what he described to us; not necessarily, however, that the occurrence which he described actually took place. Convinced, already, that he is not deceiving us, we next question whether he may not be deceived himself. Let us, however, assume that, upon investigation, the circumstances are such that collusion of any kind is out of the question, and that the man is neither trying to deceive us, nor that it is possible that he himself can have been deceived by others. Even yet we have an alternative in our judgment of the event. The phenomenon may be *subjective* rather than *objective*, the result of an action upon the man's brain rather than an outstanding reality. For nothing is more certain than the occasional occurrence of such strange impressions; and the cat or the dog or the skeleton by which the patient is haunted is frequently recognised even by himself as having no external existence. Of late years we have been able to produce instances of this depraved consciousness almost at will. The author of these remarks considers it certain that the electro-biologist has frequently caused them. The unimpeachable character of the patient, combined with the fact that he has sometimes pronounced water to be wine, or a snow storm to be taking place in a room, can only be accounted for on the supposition that he has been put into a peculiar state, during which his evidence of events is utterly worthless. But beyond the bare fact, we know next to nothing of the laws that regulate this action, nor can we tell under what conditions one man is capable of influencing another, or whether a man or body of men may not be capable of influencing themselves.

To come now to the class of events which Mr. Crookes has witnessed. It is greatly to his credit that he has come forward so frankly and honestly; and since he has begun to investigate the peculiar class of facts, we are sure that he will consider it his duty to continue the investigation in such a way as to convince those men of science who may not themselves be able to take up the question—outsiders in fact. Mr. Crookes will, we are sure, not object to a few critical remarks honestly made with the sole view of finding out the truth, and we would therefore express a wish that, in order to facilitate operations the experiments should in future be conducted by only by such men as Mr. Crookes himself, and that it should always be absolutely superfluous to investigate whether machinery, apparatus, or contrivance of any sort, be secreted about the persons present. We should thus start from a higher platform, and the investigation would gain in simplicity, although perhaps something might be lost in the marked nature of the results obtained.

Allowing, however (as we are disposed to allow), that things of an extraordinary nature are frequently witnessed on such occasions, yet we are by no means sure that these constitute external realities. The very fact that the results are uncertain, and that, as far as we know, they have never yet been obtained in broad daylight before a large unbiassed audience, would lead us to suspect that they may be subjective rather than objective, occurring in the imaginations of those present rather than in the outward physical world. Nor can this doubt be removed by any precision of apparatus; for what avails the most perfect instrument as long as we suspect the operator to be under a mental influence of the nature, it may be, of that which is witnessed in electro-biological experiments? The problem is, in fact, one of extreme difficulty, and we do not see how it admits of proof, provided the influence cannot be exerted in broad daylight and before a large audience. There is, however, a cognate phenomenon which admits of easy proof. We allude to clairvoyance, and have in our mind at the present moment a man of science who if not himself a clairvoyant has yet the power to command the services of one who is. Now, were he at once to communicate to a journal such as NATURE, in cipher if necessary, the knowledge derived through the influence, giving the proof afterwards when obtained in an ordinary manner, the public would soon be in a position to judge whether there is any truth in the influence or not.

It is, in fact, somewhat hard upon the writer of these remarks and some others who are disposed to allow the possibility of something of this nature, but have not the opportunity of investigating it, that those who have will not satisfy the public with a convincing proof.

B. STEWART

TYNDALL'S FRAGMENTS OF SCIENCE

Fragments of Science for Unscientific People. By Prof. Tyndall, LL.D., F.R.S. (London: Longmans: 1871.)

THIS volume is a reprint of a number of detached essays, lectures, and reviews, by Prof. Tyndall, published at various times and in various places during the last ten years. Besides a few shorter pieces collected at

the end, there are in all thirteen articles. These consist of two classes of a totally distinct nature. The larger number constitute considerably the greater bulk of the volume, deal entirely with scientific subjects, and are of a special scientific nature. The remainder deal either directly or indirectly with the question of the opposition or concordance of science and religion. To this question, Prof. Tyndall brings that same remarkable clearness and definiteness of statement which characterises his writings on purely scientific subjects. It is a highly desirable thing for all parties that it should be distinctly stated what are the issues, in their ultimate form, to which our various hypotheses may lead. Prof. Tyndall, from the scientific side, makes this statement clearly and distinctly. He views an hypothesis, so to speak, in its widest generalisation, and does not shrink from it or its consequences. If, he would say, you hold these or those views, then this is what they *must* imply, and what, if these views be true, you *must* come to; and so you need not be afraid, and if you hide it from yourself you only cloak the truth in the one case, or hinder the exposure of error in the other. As an example, let us take the statement of the Natural Evolution hypothesis in the lecture on "The Scientific Use of the Imagination" (page 163 of the present volume). Speaking of the evolution of the present world from a nebulous mass he says :—

"For what are the core and essence of this hypothesis? Strip it naked, and you stand face to face with the notion that not alone the more ignoble forms of animalcular or animal life, not alone the nobler forms of the horse and lion, not alone the exquisite and wonderful mechanism of the human body, but the human mind itself—emotion, intellect, will, and all their phenomena—were once latent in a fiery cloud. But the hypothesis will probably go even further than this. Many who held it would probably assent to the position that at the present moment all our philosophy, all our poetry, all our science, all our art—Plato, Shakspeare, Newton, Raphael—are potential in the fires of the sun. We long to learn something of our origin. If the Evolution hypothesis be correct, even this unsatisfied yearning must have come to us across the ages which separate the unconscious primeval mist from the consciousness of to-day. I do not think that any holder of the Evolution hypothesis would say that I overstate it or overstrain it in any way. I merely strip it of all vagueness, and bring before you, unclothed and unvarnished, the notions by which it must stand or fall."

"Fear not the Evolution hypothesis," he says further on, "steady yourselves in its presence in the ultimate triumph of that truth which was expressed by old Gamaliel when he said, 'If it be of God ye cannot overthrow it; if it be of man it will come to nought.'" This is the true scientific spirit; and the beautiful daring with which Prof. Tyndall launches upon an unknown sea trusting to this guiding principle, is an instance of that noble faith which has lived through all phases of the human mind alike in scientific and unscientific ages. To have a faith in something seems to be the ultimate necessity of all humanity. Let all of us beware how we call that faith, as it exists variously in each of us, false.

Prof. Tyndall always writes in a beautiful, clear, and pointed style. Not the least excellent part of it, and that which probably as much as anything else constitutes him the great scientific teacher he is, is his unbounded power of apt illustration. He carries this into every subject with which he deals. As an example, take the following from page 58 of the article on "Miracles and Special Providences :"—

"The mind is, as it were, a photographic plate, which is gradually cleansed by the effort to think rightly, and which when so cleansed, and not before, receives impressions from the light of truth."

Again, at page 101 we find the following :

"We live upon a ball of matter 8,000 miles in diameter, swathed by an atmosphere of unknown height. This ball has been molten by heat, chilled to a solid, and sculptured by water."

There is the touch of a master's hand in the way in which these few words "fling us the picture of the fight," and enable us vividly to realise that which they would have us realise.

Prof. Tyndall, however, has evidently given less attention to spiritual than to natural questions. Indeed, it is not to be wondered at that a man now-a-days should not have time to pay attention to everything. It is sometimes, however, to be lamented, though perhaps hardly to be wondered at, that a man should write about too much. The articles of a purely scientific character consist of two on "Radiative and Radiant Heat," one on "The Light of the Sky," and one on "Dust and Disease."

The articles on the "Life and Letters of Faraday" will well repay the perusal of those who have not already read them in the *Academy*, and will even well merit a re-perusal, as everything does which gives us any insight into the character of that great and child-like man.

The last of the series is a lecture on Magnetism, addressed to the teachers of primary schools, at the South Kensington Museum. Prof. Tyndall tells us, in a short introduction to it, that he had at first some doubts as to the propriety of its insertion. "But, on reading it," he says, "it seemed so likely to be helpful that my scruples disappeared." We are exceedingly glad that it has been so. The lecture is a beautiful example of true teaching, and of that excellent union of logic and experiment which is the true education which physical science is so well calculated to supply.

JAMES STUART

DALL'S BRACHIOPODA OF THE UNITED STATES COAST SURVEY

Report on the Brachiopoda obtained by the United States Coast Survey Expedition, in charge of L. F. de Pourtales, with a Revision of the Craniidæ and Discinidæ. By W. H. Dall. (Bulletin of the Museum of Comparative Zoology, at Harvard College, Cambridge, Mass.) With two plates. (Cambridge, U.S., 1871, 8vo.)

THIS is another important instalment of the published results of the deep-water dredgings made by our Transatlantic cousins and friends in the Gulf of Mexico. The first was issued in 1869, and consisted of a Preliminary Report on the Echini and Starfishes, by Prof. Alexander Agassiz. A report by Dr. Stimpson on the Crustacea procured in the same expedition is announced as nearly ready; and that distinguished zoologist has also undertaken the still greater charge of a report on the Mollusca. It is impossible to over-rate the impulse which will be every where given by such explorations to the study of marine Natural History.

We are now entering on quite a new phase of research, and commencing a survey of the hitherto unknown world beneath the waters. Regarded not merely in a biological, geological, or physical aspect, but also as a basis of sound education, these investigations ought not to be neglected by any civilised nation, especially by Great Britain, which, it is hoped, will never cede her well-earned maritime prestige, and her laudable ambition of discovery. This has been forcibly urged as a duty on the Government in an admirable article which appeared in the *Spectator* of the 22nd of July. In the pages of NATURE (meaning, of course, the present periodical, and not the mythical book to which fanciful writers are wont to allude), some of the results obtained in our deep-sea explorations of the North Atlantic and Mediterranean have been already noticed; and next year will in all probability inaugurate an expedition on a more extensive scale, and worthy of this rich and intellectual country. Sweden has performed her part most nobly, by sending out, in 1869, the *Josephine* frigate for the exploration of the sea-bed lying between the coast of Portugal and the Azores, and this year a corvette and tender to Baffin's Bay and Davis's Straits. Russia despatched, last year, a frigate to New Guinea for a similar purpose, under the scientific charge of an experienced naturalist, Mr. N. M. v. Maclay. We are now informed on good authority that Drs. Noll and Grenacher, two German naturalists, are projecting a dredging expedition along the coasts of Portugal and Morocco to the Canaries. Even France, in the midst of her troubles, devoted some of her energy and vast resources to the peaceful object of dredging in the lower part of the Bay of Biscay, under the personal superintendence of the Marquis de Folin, the Commandant at Bayonne. In Canada a Government schooner has been lately placed at the disposal of the Natural History Society of Montreal for dredging the deeper part of the Gulf of St. Lawrence. But the United States, not content with the laurels she had gained in the Gulf of Mexico, has this year promoted two separate expeditions; one, under the charge of the veteran and celebrated Professor Louis Agassiz, and Count Pourtales, to proceed along the south-eastern coasts of the Atlantic from Bermuda, through the Straits of Magellan to the Galapagos and San Francisco, dredging all the way; and the other, under the charge of Mr. Dall, the author of the Report above cited, has already gone from California to the Aleutian Islands.

The Brachiopoda, which form the subject of the present Report, are usually considered an abnormal class of the Mollusca; although some systematists place them in another group or sub-kingdom, the Molluscoidea, along with the Tunicata and Polyzoa. Mr. Morse, an American naturalist, has recently endeavoured to show that the Brachiopoda are Annelids. This is a very debateable matter of classification. I am, for one, disposed to let the Brachiopoda remain among the Mollusca, to which they appear to be allied through the *Anomia* family. Their mode of reproduction, bivalve shells, and general habits, evince a much closer affinity to the Conchifera than to the Tunicata, Polyzoa, or Annelida. Other points of resemblance between the Brachiopoda and the three last-named groups may savour of analogy, not of homology. The author has executed in a most scientific and conscientious spirit the somewhat difficult task allotted to him; and he has contributed much valuable information to our scanty knowledge of the life-history of these remarkable animals. I regret that I cannot accept his conclusions as to the difference of certain so-called species (*Terebratula vitrea* and *cubensis*, *T. septata* and *floridana*), nor as to the generic value of *Terebratulina* and *Waldheimia*. But this is not the place for discussing such questions. That part of the Report which treats of the *Craniidæ* and *Discinidæ* is equally well done, and the plates are capital.

J. GWYN JEFFREYS

OUR BOOK SHELF

The Year-Book of Facts in Science and Art. By John Timbs. Pp. 288. (London: Lockwood and Co. 1871.)

Annual of Scientific Discovery, or Year-Book of Facts in Science and Art for 1871. Edited by John Trowbridge, S.B., aided by W. R. Nichols and C. R. Cross. Pp. 349. (Boston: Gould and Lincoln. London: Trübner and Co. 1871.)

THE opinion that we expressed on a former occasion regarding the relative value of these Year-Books, remains unaltered. Mr. Timbs, as of old, still wields the scissors and the paste-brush with unabated zeal, and his Year-Book for 1870 presents all the faults of its predecessors. Considering that "Science and Art" are not the only subjects to which Mr. Timbs devotes his attention, but that a new book on (we may almost say *of*) "Popular Errors," or on "Curiosities," seems to be always springing from his fertile pen, his "Year-Book" does him no discredit, although non-critical readers may wonder at some of the "Facts," as well as at some of the omissions, which they encounter. Why he should place "The Entozoa Egg," (on which we suspect his ideas are somewhat obscure,) "Protoplasm," the "Germ-Theory of Disease," and "Sleep," under the head of "Natural Philosophy;" or "Snuff-Taking: a Preventive for Bronchitis or [and?] Consumption," under that of "Chemical Science," we cannot pretend to say; but, possibly, the following paragraph, taken from the heading "Astronomy and Meteorology" may afford a clue to his mysterious system of classification:—"Dr. F. G. Bergmann has projected from his own consciousness the beings from which the human race developed itself. Their name is 'Anthropiskes,' and they lived in Central Africa. They developed out of apes," p. 265. The appalling idea cannot be repressed that the intellect of our venerable instructor in "Science and Art" must be failing from over-work, so as to lead him to confound Anthropology with Astronomy!

The American Annual has the great advantage over its British rival of being compiled by men who understand the subjects on which they are engaged. The editor, John Trowbridge, S.B., is Assistant Professor of Physics in Harvard College, and one of his assistants, W. R. Nichols, is Assistant Professor of Chemistry in the Massachusetts Institute of Technology. The subjects embraced in this volume are nearly the same as those included in Mr. Timbs's Year-Book: Mechanical and Useful Arts, Natural Philosophy, Chemistry, Natural History or Biology, Geology, and Astronomy and Meteorology, being common to both, while the present work has additionally Geography and Antiquities, and the English annual makes Electricity a separate subject.

Unless the editor enters more fully in future volumes into the subject of "Geography and Antiquities," we should recommend the suppression of this department. On the present occasion it is simply compiled from the proceedings of the Geographical Section of the British Association,

and does not contain a reference to *Petermann's Journal*, to the French or German Geographical Societies, nor even to the American Geographical Society.

With this exception the "Annual of Scientific Discovery" is entitled to our earnest commendation. The editor and his assistants have done their work well, and the only editorial slip that we have noticed is the insertion of the same paragraph in two separate departments (see pp. 122 and 208). The "Notes of the Editor" at the commencement of the volume are, as in preceding years, especially deserving of praise, and indicate in a comparatively short space the progress of science for the year.

Mycological Illustrations, being Figures and Descriptions of New and Rare Hymenomycetous Fungi. Edited by W. Wilson Saunders, F.R.S., F.L.S., and Worthington G. Smith, F.L.S., assisted by A. W. Bennett, M.A., B.Sc., F.L.S. London large 8vo., tab. lith. pict. 24. (London: John Van Voorst, 1871.)

THOSE who have made the longest and most intimate study of Fungi are most sensibly alive to the fact that it is almost impossible to name species, especially those belonging to the genus Agaricus, without figures derived from the authors themselves to whom they are attributable, or at least made under their immediate inspection. It was therefore a great boon to mycologists when Prof. Fries, a student of some sixty years' standing, determined to deposit in the museum at Stockholm figures of a large portion of those species, described by himself, which have a softer texture, and are with difficulty preserved for the herbarium; copies of many of which, and frequently the original sketches, have from time to time been kindly transmitted to this country, while the illustrations themselves are in the course of publication. Five fasciculi have already appeared under the title "Icones selectæ Hymenomycetorum nondum delineatorum," containing fifty plates, several of which comprise two or more distinct kinds; and it is much to be hoped that increasing years will not prevent the venerable mycologist from continuing his indispensable work, supplementing, as it does so nobly, the "sveriges ätliga och giftiga svampar," which furnishes a hundred plates, of which several are critical species, though, from the nature of the publication, the greater number are well-known forms.

We have now before us a work of much importance in the same direction, which, though not sanctioned by so long a study or such numerous treatises, must ever be of considerable weight from the unusual artistic talent of Mr. Worthington Smith, to whom, in conjunction with Mr. Wilson Saunders, the illustrations are due. He has not, however, rested entirely on his own knowledge of the subject as regards the determination of species, but has very wisely obtained help where it was possible to do so. In general the species are very correctly determined, but we venture to make one or two observations where some doubt exists, a matter of no surprise in so very complicated a subject.

Fries has just published a figure of his *A. polius* which is very different from that in the work before us, and which agrees with what we have ourselves always considered that species. *Boletus pachypus* is certainly not the plant of Fries as figured in his work on esculent and poisonous fungi. We have no right to criticise *A. junonius*, as it has the sanction of Fries himself, but we cannot help remarking that it does not at all resemble the figure in the "Svensk Botanik." As regards Cortinarii it is most desirable that the young state should always be figured. *Cortinarius caninus*, for example, is much brighter in colour at first. The figure clearly represents an older condition. The least satisfactory figure is that of *A. hydrophilus*, which differs from the usual form in not having a fistulose stem. There are some errors, whether clerical or otherwise, which call for a stricter revision in future numbers of the Latin phrases.

Thirty species are illustrated in the twenty-four plates, the figures for the most part leaving nothing to be desired. Far the greater part of them have either not been figured before, or the published figures are not satisfactory. We may mention as peculiarly good *Cantharellus radicosus*, *Agaricus atro-cæruleus*, which reminds us of Gould's drawings of infant coots and waterhens; *A. lignatilis*, and *Gomphidius glutinosus*. We trust that this very useful and acceptable work will command such a sale as to ensure its continuance. The materials in the hands of the editors are almost inexhaustible, and are daily increasing.

Since the above was written, a letter has been received from Prof. Fries containing some kindly worded criticisms, the most important of which are subjoined. The least observation from a person of such wide experience must be welcome to every genuine mycologist, and to none, we are assured, more so than to the authors of the work before us. *Cortinarius callisteus* = *A. ferrugineus* Scop., agreeing exactly in habit with the plant of Fries but differing in colour. *A. polius* = *A. fumosus*. *Boletus pachypus* = *B. amarus* Fr. *C. cærulescens* = *C. cumatilis* Fr., *species valde variabilis*. He adds, "the price is so moderate that it excites my admiration. Your admirable work has been received with singular pleasure. It contains three interesting species quite new to me: *Cantharellus radicosus*, *Agaricus adnatus*, and *Agaricus polystictus*."

M. J. BERKELEY

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Mr. Howorth on Darwinism

WILL you allow me to reply to the various letters which appeared in your last number in answer to one from me? I gratefully welcome their general courteousness. Postponing the consideration of Mr. Wallace's letter, I come to Dr. Lionel Beale, the relevancy of whose arguments, and especially of the lugubrious moral attached to them, I fail to understand. It seems to me to be so incoherent and rhetorical that it is far beyond the reach of reply.

Mr. Tylor refers to the last census as disproving my position. He says the population has increased enormously, and yet our age is characterised by its luxury. These statements are correct. But the argument deduced from them has a missing link. The luxury of the upper strata of society has increased with its wealth, but the numbers of the pauper class have been increased in the same rate. In considering the published returns of the Poor Law Board, I am compelled to admit that the increased luxury has been limited to the surface of society, and that its lowest ranks have been correspondingly recruited, and to admit the force of Mr. Doubleday's argument, that the population of England under the Tudors was stationary because of the generally diffused wealth, while that of Ireland in the last century was increasing at an enormous rate, because it was steeped in poverty and want. I am not arguing about individual cases, but about general laws. Now, in Lancashire, where the increase has been so marked, I have it on the authority of owners of mills that the indigenous stock of the county, which is thrifty and well off, is not an increasing element, but is being replaced by the children of the Irish, or semi-Irish blood, from the poorer quarters of the large towns, among whom prudential restraint (which is surely a very visionary *causa causans* in any event) cannot be said to have much influence. At Rome, Venice, Basle, and in France, where the aristocratic class was not limited by primogeniture, it was always dying out, and was only recruited by fresh creations (see the details in Doubleday, chapter iv. *passim*). In all these cases we can appeal to figures, and not to a superficial survey of a Peerage, or the limited area of our own acquaintance.

The particular passage quoted by Mr. Tyler from Malthus has been conclusively answered by Doubleday (chapter vi.), and it is useless to repeat his arguments, which on this point I consider to be unanswerable.

Mr. Lownes repeats the odd charge of Mr. Tait against me, that I put the cart before the horse. The latter gentleman, whom

I have not yet answered, cited against me the elementary case of capons and other creatures of that ilk. They are entirely beside the question. It is as reasonable to quote them in this discussion as to conclude that all chaste people must be cowardly and effeminate because mutilated animals are so. He also said that I mistook the whole rationale of the question, and that it is infertile creatures that grow fat, and not fatness that causes sterility. The only test of the question is the one I have not shrunk from applying in this argument (which, by the way, has not to do so much with the fat as the hearty and strong). This test is that in a great number of cases we can make strong and vigorous but sterile plants and animals fertile by starving or bleeding them, which proves that it is not the organs that are defective, but that the creatures are too hearty.

The experience of Mr. Lownes on the fecundity of consumptive patients, and of the poorest classes as compared with the richest, is at issue with that of the doctors and midwives whom I have access to, and of all the authorities I know whose opinions are based upon statistics.

I am not sure that I understand the second and third paragraphs of his letter. Whichever way the problem is put, I am satisfied if it be admitted that in the more crowded and squalid portions of our towns, the population as a rule is more fertile than in the less crowded neighbourhoods. The case he cites of poor women losing their children early and ceasing to give milk, and, in consequence, soon becoming pregnant again, is counterbalanced by the fact that among the richest the proportion of those who suckle their children is small, and this not because of fastidiousness, but because they secrete little milk. Mr. Lownes once more drags out the Indian and the backwoodsman, but he has overlooked the answer I gave to Mr. Wallace in my former letter, which needs no alteration to meet the case as he has put it. It is the case of the meat-eaters against the vegetable-feeders, the strong and hearty and active against the comparatively stolid and low-conditioned, and as in such cases all the world over the former are not so fertile as the latter. Mr. Lownes objects to savages being cited, because of qualifying circumstances; he may as well say that it is not fair to test natural selection by wild animals, but only by domesticated ones. His treatment of the case of the Patagonian women is convenient but flippant. Mr. Lownes' experience in breeding both cattle and sheep and fowls and in rearing plants must be extremely limited, or he would hardly have made so rash an assertion as that contained in his last sentence. The starving of plants and animals to induce them to breed is one of the elementary axioms of both gardeners and stockkeepers.

I now come to Dr. Ross's letter, which, although somewhat patronising in parts, is altogether more to my taste than some others. He has properly referred me to Mr. Herbert Spencer, but I am afraid of venturing into his book, for fear that I should open upon myself the floodgates of Evolution. It is not the general problem of Evolution about which we are now at issue, but that limited form of it called Natural Selection. It is satisfactory, however, to find that, according to Dr. Ross, Mr. Herbert Spencer admits the main facts upon which my argument is founded. His doing so is quite a relief after the jaunty manner in which some of your correspondents have spoken about the matter. To speak of its being late in the day to be now defending Mr. Doubeday, to tell one that "what one says is ludicrous," "a monstrous error," &c., &c., is surely a sign that the crowing of the Gallic cock has been mistaken for more substantial arguments. I am very sorry that Mr. Spencer's book is not in my library, and that I cannot meet with it at the Manchester Free Library or Mudie's, so that until I am aware of Mr. Spencer's arguments I cannot say how far they affect the position I maintain. If the facts are admitted, as Dr. Ross says they are, I confess that I cannot see any other interpretation of them than the one given by Mr. Doubleday. Will Mr. Ross do me the favour of pointing out what other explanation they are capable of?

Mr. Wallace has misunderstood me if he thinks me capable of sneering at the good and sound work that has been done by himself for many years, the value of which I am as conscious of as I am of the worthlessness of mere Olympian dogmatism. Sneers are only justifiable in answer to contempt, and if he feels aggrieved with any of my words I withdraw them.

Mr. Wallace says my criticism of the phrase Survival of the Fittest is satisfactory. In regard to the phrase I used, and for which I was severely flouted by Mr. Wallace, he says it is unknown to Darwinians; that may be, but it can hardly be said to be unknown to Mr. Darwin himself. Speaking of the problem of the conversion of varieties into species, the latter says: "The inevitable result is an ever recurrent struggle for existence. It has been truly said that all nature is at war, the strongest ultimately prevail, the weakest fail, and we well know that myriads of forms have disappeared from the face of the earth" ("Variation of Animals and Plants under Domestication," i. 5). Let me especially commend this extract to Dr. Lionel Beale, for whom I entertain the profoundest respect, notwithstanding his vituperation of myself.

I find a difficulty in meeting Mr. Wallace's latest arguments, because they are entirely *a priori*, and Mr. Wallace asks me to admit as premisses the very thing I dispute, namely, the relative sterility of strong and hearty animals and plants. I cannot see the relevancy of his quotation of the effects of cross-breeding to the present argument, unless he means to infer that crosses are more vigorous and stronger than pure bred animals, on which position I should like to be furnished with a little evidence. Again, I cannot test the suppositious problem put by Mr. Wallace as to the strongest individual of an animal's progeny eventually being the stem-father of the race. He takes for granted that it is, and in doing so begs the question. I can only say the only experiments I know do not favour Mr. Wallace's *a priori* view, and that in the cases we can experiment upon, not the least satisfactory of which is the case of man himself, the condition most favourable to fertility, as I have quoted many examples to show, is that of comparative depletion.

Mr. Wallace, as before, is spare of instances. I can only extract two *bonâ fide* ones from his letter. He tells us the strongest bull leads the herd; this proves nothing, unless we are to infer from it that his progeny is the most numerous, and that the biggest and strongest therefore survive. I prefer to quote Mr. Darwin himself where I can. If Mr. Wallace's instance be worth anything, how does he account for the following: "The decrease in size of the Chillingham and Hamilton cattle must have been prodigious, for Prof. Rutimeyer has shown that they are almost certainly the descendants of the gigantic *Bos primigenius*. No doubt this decrease in size may be largely attributed to less favourable circumstances. Yet animals roaming over large parks and fed during severe winters can hardly be considered as placed under very unfavourable conditions" ("Variation of Animals and Plants under Domestication" ii. 119). What Mr. Darwin says of the wild cattle is equally true of the reindeer kept by the Laplanders compared with the wild ones on the Samoyede tundras, of the red deer of our larger forests compared with the skeletons of red deer from the turbaries, and is, perhaps, generally true of semi-wild races where man has not intervened with the special object of increasing the size by breeding from the largest individuals only.

In regard to the carnivora, I know of no reliable facts. I am not proposing the monstrous paradox that those animals which are so weak, diseased, or decrepid that they cannot sustain life at all, are the only ones that keep up the succession of the animal world. The toothless tigress, who cannot kill her food and is starving, will most certainly not be the mother of a long race. She can do nothing but die. But I say that, judging from analogy, it is probable that the lean and comparatively ill-fed tigress will breed more freely than the man-eater supplied with regular and abundant food.

The banks of the Chinese rivers and the rough country in the south and south-west of Ireland are both inhabited by teeming populations, remarkable for their poverty and fertility, and remarkable further for sending out immense colonies, which supplant wherever they go, in Mantchuria, in Songaria, in Glasgow, in Manchester, in New York, the strong hearty, indigenous races. This being so (and I only quote these two as examples of a whole class), when Mr. Wallace asks the question, "How can weak and sickly parents provide for and bring up to maturity their offspring, and how are the offspring themselves (undoubtedly less vigorous than the offspring of strong and healthy parents) to maintain themselves?" I can only reply that they actually do so: *Veni, vidi, et credi.*

I must correct a wrong impression that Mr. Wallace has got hold of. In this controversy I have no theory; my only theory is that Natural Selection is an ingenious but fallacious explanation of the varieties of life.

I cannot understand Mr. Wallace's last sentence if it be meant for an argument; while if it is only a *jeu d'esprit* and witticism, it requires a commentary to tell us where the point is.

Lastly, I will consider Mr. Wallace's reiterated complaint that I have only treated of what is in most cases the least important factor in determining the continuance of species. Let me turn

very briefly to another of these factors put prominently forward by both Mr. Wallace and Dr. Beale, namely, "Obscure Colour."

We are not arguing about exceptional and individual cases, we are dealing with a general law, applicable or supposed to be applicable to the great majority of cases. Can it be said gravely that obscure colour has tended to the preservation of particular forms of life to the exclusion of others, not in a few exceptions, but as a general biological law?

Daylight, it will be admitted, is more likely to disclose an object than darkness. If we compare diurnal forms of life with nocturnal ones, we ought to find, if I read the tendency of the Darwinian argument rightly, that in the daylight when a sombre, obscure, or indifferent colour, would be of great service to hide an object, that there are a much smaller proportion of conspicuous forms of life abroad than at night when there would be no such need for obscurity, and a bright colour might be worn with impunity. Is such the fact?

Again, if we compare the animals and plants that live in tropical climates, where the light is intense, with those found in temperate and severe ones where the light is not so great and objects are not so prominent, do we find that the former has a comparative monopoly of conspicuous objects, or do we find rather that the reverse is the case, and that all the brightest objects we know in nature—the parrots, macaws, humming birds, butterflies, orchids, &c.—are found in the greatest profusion in the tropics, while we proverbially console ourselves for the absence of colour in our birds by boasting of their singing, and hang the beetles of Brazil in necklaces round our sisters' and wives' necks, while we crush our sombre representatives of the same class under our heels? Is it not equally true of the sea? In the Mediterranean, for instance, do not the brightly decked out gurnards and mullets far outnumber the dingier fish, while on the banks of foggy Newfoundland the sober tinted cod and ling are the prevailing types? In the former we have the clear blue water that washes round Sorrento pierced through and through by the blazing sun, while in the latter we have everything gloomy except the fisherman.

If we separate the animal world into flesh eaters and vegetable eaters, we ought to find, if this theory be true, that the former (which as a rule are not themselves the prey of other animals) are more conspicuous than the latter, since they have less reason for adopting a secret costume. But is it so? Are the hawks and owls and carnivorous beetles as classes more conspicuous than their victims? Is it a not fact that the most beautifully coloured creatures are as a rule the most helpless, weak, and accessible; that those animals which are supplied by nature with weapons of defence or are strong and can defend themselves, are as classes more obscure in colouring than those not so protected, and that the same rule applies to plants which are poisonous, nauseous, or protected by thorns? If these facts be true in the great majority of cases, we have another factor in Mr. Darwin's theory which is not satisfactory, and the cases quoted to support it become mere exceptions, which, by being exceptions, disprove the particular law he is maintaining. This letter has already exceeded reasonable limits, and I must postpone a further consideration of this and other objections to another occasion.

Derby House, Eccles HENRY H. HOWORTH

MR. HOWORTH'S objections to the theory of Natural Selection have been fully answered. I therefore wish to direct attention to another objection which has been recently advanced, and which has not, so far as I know, been specially refuted. The objection is stated by its author in the following terms:—"And it has been affirmed that to 'the primitive properties of molecules' and 'Natural Selection' may be referred all the varying forms and structures known to us, as well as all the phenomena of the living world. But such terms explain nothing. By their use further inquiry is discouraged, and the mind bent upon investigating the secrets of Nature is misled at the very outset. Can any one of these very pretentious phrases be resolved into anything more than the statement of a fact or facts in the form and language of an explanation? Natural Selection is the formation of species, and species are produced by Natural Selection. Crystallisation is the formation of crystals, and crystals are produced by the operation of crystallisation."

This passage is extracted from p. 58 of "The Mystery of Life"—a little work by Dr. Beale, which was published a few months ago. Dr. Beale has a keen appreciation of the "ludicrous." He thinks Mr. Howorth's misrepresentation of the Darwinian theory "very curious and even ludicrous," and in the closing sentence of his letter in NATURE, he appears to have a bit of fun to himself which ordinary mortals cannot understand; and if he can prove that Natural Selection is a mere abstract statement of the fact that species are in some way or other formed, the Darwinian theory is the most "ludicrous" ever presented to mankind. Probably Mr. Wallace may take a different view of the subject, and he may even think that the objection is more ludicrous than the theory; at any rate, no harm can result from bringing Dr. Beale and the champions of Natural Selection face to face, so that stricter tests than the "ludicrous" may be applied to ascertain whether the truth lies in the theory or in the objection. JAMES ROSS

Newchurch, July 24

THE last paragraph of Mr. Howorth's letter in NATURE of July 13 reminds me of a fact which I have often noticed, and which is, I suppose, well-known to botanists, viz. that certain creeping plants which root at the joints, flower sparingly unless the sprays are so disposed that they cannot take root. I refer especially to the *Lysimachia nummularia* (larger moneywort or "Creeping Jenny"). This plant blossoms comparatively little when allowed to trail in the moist soil which is its natural *habitat*, and in which alone the leaves look healthy and thriving. A spray trained off the flower bed on to a flag-stone, or a plant grown in a pot so as to hang over the edge and not be able to take root, will look sickly, but will be covered with flowers. I think I have noticed the same thing in connection with the periwinkle.

Gardeners cut off the runners of strawberries and the suckers of fruit trees to increase the crop, because, as they say, runners exhaust the plant.

But is not the case, rather, that the possibility of continuing its own life by taking root at the runners makes the plant's constitution, as it were, lazy about propagating its kind?

It is, perhaps, worth noticing that the cutting off the runners or suckers does not in any way weaken the plant, or cause it to become sickly, but it does prevent the indefinite prolongation of the individual life.

THE OWNER OF A "WEED GARDEN"

Recent Neologisms

WRITING, as I did, from a little Midland village, where access to an English dictionary was impossible, I am not surprised to find that three words, which I treated as recent coinages, were only re-introductions. *Survival*, *impolicy*, and *indiscipline*, are all so naturally formed, that, whether old or new, they are "welcome to stay." My end was answered by putting a brand on Mr. Wallace's *prolificness*, by way of contrast. If he is bent on using that monster, he will help to naturalise it by spelling it with *ck* (instead of *c*) like *thickness*. But surely he is not driven between the Scylla and Charybdis of *prolifickness* and *prolificacity*, when *prolicity* is staring him in the face. For my part, I pray that the whole family will (to quote Sylvester again) "shake swift wing," and be no more seen. By-the-bye, I find the verb to *handwrite* in the *Quarterly Review*, April 1871, p. 332. That is a good, if not a new word, and well deserves re-introduction.

C. M. INGLEBY

The British Association and Local Scientific Societies

IT is to be regretted that the British Association does not exert its influence in stimulating local scientific societies towards greater efforts for the formation in their museums of collections representing the Geology and Natural History of their respective neighbourhoods, so that they might constitute local monographs. Such a system, combined with a central museum in London, representing an epitome of the collections throughout the country, would tend to the advancement of science with greater rapidity and accuracy than at present, when the provincial museums are little better than overstocked curiosity-shops, and with no recognised plan of arrangement which is greatly wanted. In general there is little space for additions of importance, from the fact that the museums already contain large miscellaneous collections, unconnected with the neighbourhood, and of little use to anybody. Many valuable private collections exist throughout the country, representing the geology, &c., of various localities, which are eventually too often dispersed and lost to

the district where they would be most useful and instructive. Private collectors would probably show more public spirit, if greater zeal and better judgment were shown by local societies.

F. G. S.

Science Teaching in Schools

IN the number of NATURE for April 20, there is an article containing an account of a "Plan for Teaching Science in Ordinary Schools, submitted to the London School Board by Mr. J. C. Morris."

I will ask you to give me a little space for some details respecting an educational experiment I made in 1867, 1868, and 1869. My object was to test the value of a plan much resembling that referred to. By means of circulars, addressed to more than a hundred of the London clergy, I obtained permission to have the children in seven large schools instructed in science. Four competent teachers put their services at my disposal. One of these gentleman is now chemist in iron works, two are art masters, and the fourth, having obtained one of the Whitworth Scholarships, is a student at Owen's College. I mention these facts to show the sufficiency of their knowledge. Three of them had had considerable experience in teaching. Twenty-two classes were formed, the total number of pupils exceeding 800. The principal subjects taught were chemistry, geology, physical geography, practical geometry, and mechanical drawing. The lessons were from one to two hours in duration on two days in the week at each school. But my plan differed from Mr. Morris's, inasmuch as thirty-five to fifty-five lessons were generally given in a subject before proceeding to a new one. He suggests that "a single teacher could get through three or four subjects annually, so that in two or three years he would have completed the full course in each school." This plan would give from twenty-two to thirty lessons per subject if I rightly understand his meaning. We fixed a small fee, but seldom obtained it, as we found that any attempt to press for payments would have reduced very materially the numbers in the colleges. The pupils were frequently examined, and those who appeared likely to satisfy the minimum requirements of the science department were sent in to the May examinations.

The following are some of the observations I made at the time:—

1. Few of the children appeared to obtain anything like sound and comprehensive knowledge of the facts the teachers put before them.

2. The great majority failed to express clearly on paper any ideas which an oral examination showed they had gained.

3. Most of them appeared to forget a subject within a few weeks after the discontinuance of instruction, or the substitution of another branch of science. The utter forgetfulness shown by whole classes was sometimes almost startling.

4. The papers worked by the girls at the examinations were superior to those produced by the boys, showing a more intelligent knowledge of the subjects they had been taught. This fact may, however, have resulted from accident, as comparatively few girls received instruction.

T. JONES

The College, Stony Stratford

Ocean Currents

MR. LAUGHTON does not seem to observe that the subject of Ocean Currents involves several distinct issues, which may be discussed apart from each other. It is, of course, obvious that if the temperature explanation of the vertical circulation fails, then no illustration of the horizontal circulation, if founded on the temperature theory, can be really effective. But it is admissible to inquire separately whether the horizontal circulation *would* result from a vertical circulation such as the temperature theory suggests. For an objection has been urged against the theory on account of the nature of the horizontal circulation (see Herschel's "Physical Geography.") The express object of the experiment I have suggested is to show that this particular objection is unsound, or rather to illustrate the theoretical considerations argued in my essay on the Gulf Stream in the *Student* for July 1868.

But even in so far as my suggested experiment, like the similar one carried out by Dr. Carpenter, illustrates the production of a vertical circulation, I deny that Mr. Laughton's objection is valid. It is quite unnecessary to have a thermometric gradient resembling that in the terrestrial oceans. Whether Dr. Carpenter's view be correct, according to which the Arctic regions are the place where the Ocean Currents have their birth, or whether the view I have advocated be preferable, that the chief source of the oceanic circulation is to be recognised in the effects of tropical and subtropical heat, it is clear that we are rather concerned with the integrated effects of one or other cause (or of both causes combined) than with the amount by which temperature increases per mile of distance towards the equator. As I have already remarked, I conceive that any reasoning by which the contrary could be maintained would subvert the accepted and surely sufficient explanation of the trade and counter-trade winds. (The experiment described in illustration of this explanation in Daniell's Meteorology is open to much graver objections than Mr. Laughton has urged against Dr. Carpenter's experiment.) And I note that here Mr. Laughton agrees with me, except that on the strength of his thermometric gradient he is as ready to give up one theory as the other, whereas I see no objection to retaining both.

The very word "gradient" should suggest the true answer to Mr. Laughton's reasoning. A gradient of one in ten (say) will produce little velocity in a rolling body traversing such an incline for a distance of only a few feet, but if the incline be a few miles long the body rolling down it would acquire a velocity exceeding that of our swiftest express trains. Or again, suppose Dr. Carpenter, desiring to illustrate the subject of springs of water, employed a conduit-pipe inclined 45 degrees to the vertical, would it be any valid objection to the illustration to urge that in most natural springs the water gradients are very much less? He could surely answer that the principle of his illustration was in no way affected by this circumstance, for if the water-gradients in nature are small, they act over a much longer range than could be employed in his experimental illustration. So with Mr. Laughton's temperature-gradients; they are very small indeed, but their action extends over a very great distance; and as in the two former cases the total fall measured vertically is to be looked upon as the true cause of the resulting motions, so I conceive that the total difference of temperature between Polar and Equatorial waters is to be considered in discussing the temperature theory of oceanic circulation.

I note, by the way, that "solar light" (by misprint or through a *lapsus calami*) was substituted for "solar heat" in my former letter. I did not think it necessary to correct this earlier, as I imagined the error would mislead no one. Like Mr. Laughton I "do not see what effects solar light can ever be supposed to produce," on the ocean, at least, in producing circulation.

I venture to remind Mr. Laughton that Dr. Carpenter's position in this matter is very different from his or mine. *We* have theorised on this subject, whether with more or less soundness time will show. But Dr. Carpenter has brought striking and important facts to our knowledge; and *if* there has been "an air of triumph both in Dr. Carpenter's lectures and writings" about ocean currents, he has had better cause for triumph than the mere success of a lecture-room experiment could have afforded him.

RICHARD A. PROCTOR

Brighton, July 21

Western Chronicle of Science

I WOULD beg to be allowed one or two remarks with reference to the very favourable review of the "Western Chronicle of Science" which appeared in last week's NATURE.

It is not a "common Cornish habit to hang heavy jackets, great-coats, &c., on the lever of the safety-valve," and the farmers do not, *as a rule*, "mix guano with lime a few days before applying the manure." The editor has seen both these absurdities performed, and has used them as beacons to warn young men what to avoid. I may also remark that Mr. Williams's Paper is on Scientific *Mining* and not Scientific Nursing.

Falmouth, July 22 J. H. COLLINS

Formation of Flints

NOTHING can be more annoying to a reporter than to find he has not satisfied those whose statements it has been his duty to condense. I have therefore carefully examined the report to which Mr. Johnson takes exception in his letter to you of the 11th inst., and I regret that I am unable to acknowledge any error.

If Mr. Johnson will be good enough to consult some of those who were present at the meeting to which he refers, he will, I think, be more inclined to admit the accuracy of the report.

THE WRITER OF THE REPORT

NOTES

THE arrangements are now completed for the session of the British Association, to commence on Wednesday next; and we may fairly expect a successful meeting. The large number of foreign *savants* who have announced their intention of being present will add greatly to the interest of the meeting, and the inhabitants of the pleasant Scottish capital seem determined to display to the utmost their well-known hospitality, both in a public and private capacity. The President's Address, as we have already announced, will be delivered on Wednesday evening at 8 o'clock; and at the first meeting of the General Committee, at 2 P.M. on the same day, the Presidents, Vice-presidents, and Secretaries of each Section will be appointed. On Thursday morning at eleven, the different sections will assemble in the rooms appointed for them, for the reading and discussion of reports and other communications; and the sittings will be resumed at the same hour each day till Tuesday, August 8. All further information may be obtained by those wishing to be present from the local secretaries, 14, Young Street, Edinburgh.

THE Emperor of Brazil has signified his intention of being present at the approaching meeting of the British Association.

THE Indian Civil Engineering College at Cooper's Hill will be opened by the Secretary of State and members of the Council of India on Saturday, August 5th.

WE have great pleasure in recording the inauguration of an effort to raise a memorial to the memory of the late Prof. William Allen Miller, and desire to call thereto the attention of all our readers who appreciate the valuable contributions to science for which we are indebted to that eminent chemist. The committee consists of Dr. Miller's fellow-professors at King's College and fellow-labourers in science, with the Rev. Principal Barry as chairman, Profs. Bentley and Bloxam, and Messrs. Cunningham and Tomlinson as secretaries, and Prof. Guy as treasurer. The intention is to raise a fund to be devoted, first, to the preparation of a bust or portrait of the late Dr. Miller, and, secondly, to the institution of a prize or scholarship in connection with King's College, and bearing his name. The ordinary amount of subscription is to be one guinea, and the list of subscribers will be published without any statement of the amounts subscribed.

THE International Congress of Prehistoric Anthropology and Archæology, which was last year postponed on account of the war, will be held this year at Bologna under the presidency of Count Gozzadini, and with Prof. Capellini as organising secretary. The sittings will commence on the 1st of October, and will continue during the following week. Mr. John Evans, F.R.S., of 65, Old Bailey, has consented to receive the subscriptions of English members, the amount of which has been fixed at ten shillings, and the payment of which entitles the member to the volume of proceedings.

THE President and Council of the Royal Geographical Society have addressed a letter to the Vice-Chancellors of the Universities of Oxford and Cambridge on the subject of the teaching of Physical and Political Geography. They observe that in the scheme now under the consideration of the Universities for the examination of boys between the ages of sixteen and eighteen from all the first-grade schools of England, neither branch of geography is included in the list of subjects out of which the boys are at liberty to choose any five for examination. They point out that geography has always been regarded as an essential, though subordinate, element of liberal education, and that it has been more and more frequently selected as their subject by candidates who pass the examination of the Science and Art Department of South Kensington. They hope that the Universities may see reason to repair the omission in the scheme above alluded to, and that they, by this and other means, will not only rescue geography from being badly taught in our schools, but will raise it to an even higher standard than it has yet attained.

THE examiners in the School of Law and Modern History at Oxford have given notice that at the next examination in December, Geography will form an important branch, and that papers will be set in the Honours Examination on this subject alone.

BY the appointment of Mr. Alexander Herschel to the Professorship of Experimental Philosophy at the Newcastle College of Physical Science, a vacancy occurs in the chair of Natural Philosophy at Anderson's University, Glasgow. Applications must be sent to the secretary by the 26th of August.

IN a letter to the *Athenæum*, the widow of the late Prof. De Morgan invites those who possess letters or other mementoes of the illustrious mathematician to lend them for the purpose of preparing a biography.

PROF. MARSH, of Yale College, has just started out on a second expedition for scientific exploration and discovery in the far West, which we trust will be still more fruitful in interesting results than the first one which brought to light so many extraordinary forms of fossil animals, that have been briefly described by him in the *American Journal of Science*, and referred to from time to time in our pages. His party for the present season will consist of thirteen besides himself, embracing quite a number of his companions of last year, and it is his intention to spend five or six months in searching the cretaceous and tertiary strata of the Rocky Mountain region and the Pacific coast for vertebrate fossil remains. With the experience of the past year and ample facilities, he expects to make very extensive collections.

THE New York Commissioners of Fish and Fisheries seem unwearied in their efforts to stock the waters of the State with the best varieties of fish. Among other results obtained by them, has been the hatching out during the past season of 3,000,000 shad eggs, or three times the total catch of the Hudson River. They have also bred several millions of white-fish, a million of salmon-trout, while of such fish as the black bass, pike, perch, and other varieties, they have supplied large numbers to those who would take and protect them. The period of their appointment will expire in the course of a year; but by that time, even if the commission should not be renewed, they will have made a most important impression upon the subject of the production of the fresh-water food supply.

COLLECTORS of scarce works in Natural History, curiosities, stone implements, rare specimens, &c., should not neglect the opportunity of inspecting the collection of a well-known collector, which will be sold at Thurgood and Giles's Auction Room, 7, Argyll Street, Regent Street, on July 31st and three following days; and will be on view two days before the first day's sale.

THE old adage about civilisation, or at least science, softening manners, is certainly being exemplified just now in France. M. Paul de Saint-Victor having given utterance to a violent tirade of undying hatred against Prussia, M. de Quesneville thus replies in the *Moniteur Scientifique*:—"L'humanité veut qu'on oublie; l'interêt des peuples, qui sont tous frères, la raison, le bon sens, tout nous dit que dans cette guerre qui vient de finir, la France, qui a succombé, doit chercher sa revanche, non dans la puissance de la force brutale, mais dans sa régénération sociale, et qu'elle doit demander à son génie de prouver sa supériorité dans les sciences, dans les lettres, et dans les arts, et que ce doit être là sa seule vengeance. C'est par là que la France est vraiment invincible, c'est par là qu'elle doit rester la grande nation, la nation aimée et préférée, et non dans une lutte d'obus et de chassepots." Noble words these, and full of the most rare form of generosity,

that of the vanquished towards the victors; a fitting response to the note of reconciliation given forth by the venerable Baron Liebig, to which we referred some weeks since.

NORFOLK has always been noted for its devotion to ornithology. The "Transactions of the Norfolk and Norwich Naturalists' Society for 1870-71" contains several interesting and useful papers, among which we may especially mention "On the Ornithological Archæology of Norfolk," by T. Southwell, "On a Method of Registering Natural History Observations," by Prof. Newton, "A Natural History Tour in Spain and Algeria," by J. H. Gurney, and "On Certain Coast Insects found existing inland at Brandon, Suffolk." The author of this last paper believes that these species must have survived for several thousand years, since the great valley of the fens was submerged. The insects found are peculiar to coast sand-hills, the nearest of which are at a distance of forty miles; and yet, "in spite of their isolation and alteration of condition, the species are as true and as clearly defined as those of our present coast."

MR. W. G. M'IVOR, Superintendent of the Cinchona Plantations of the Bengal Government in British Sikkim, has published a lengthy report, of which the following is an abstract:—"The plantations are situated in the Valley of Rungbee in the Himalayas, about thirteen miles frem Darjeeling, which seems admirably adapted for the growth of cinchona. The climate is very moist, being rarely free from rain. Nevertheless the state of the plantations is reported as very unsatisfactory; the plants have nothing like the luxuriant foliage which characterises those grown in Southern India on the Nilgheries. They seem to thrive for three or four years at the most, and then become diseased." Mr. M'Ivor says that trees of equal height do not produce so much bark as in the South of India, being of more slender growth, and the bark being thinner.

A GREAT demand for the English sparrow in various parts of the United States has induced their importation from England and Germany in large numbers; but in many instances where this has been done in large cages, most of the birds have died on the passage. In one instance, where four hundred were placed in two cages, only seven were safely landed in New York. Persons who have given this subject their attention, advise that the importations be made in long low cages, known as store cages, which are two or three feet long, about nine inches high, and twelve from back to front, with perches within two inches of the bottom. In a cage of this kind three or four dozen can, it is said, be readily transported, provided they be supplied with proper food, as well as with sand and fine gravel and plenty of water.

M. WURTZ has announced to the French Academy of Sciences that a young chemist in his laboratory has succeeded in transforming lactose, or the uncrystallisable sugar of milk, into dulcose or dulcine, the sugar of mannite, which may easily be obtained in very beautiful crystals, by the successive reaction of hydrochloric acid and sodium-amalgam.

M. FELIX PLATEAU has recently undertaken a number of experiments to determine the question whether the cause of the death of fresh-water animals when removed to sea water, and of marine animals when removed to fresh water, is the difference in the density or in the chemical constitution of the water. His observations were made mostly on various species of Articulata; he found that those fresh-water species which possess an aërial respiration can survive the change to salt water, while those which possess only a branchial and cutaneous respiration die quickly. By experimenting on water made denser by the solution of sugar, M. Plateau came to the conclusion that the density of the water is not the destructive agent, but a portion of the salts held in solution. The chlorides of sodium, potassium, and magnesium, he found to be very quickly fatal to fresh-water species, while the sulphates of magnesium and calcium had no prejudicial effect. In the same manner the death of marine animals in fresh water appeared due to the giving off of sea-salt from their bodies to the surrounding fluid. All these facts he believes explicable from the laws of endosmose and diffusion.

"A KEY to the Natural Orders of British Wild Flowering Plants," by Thomas Baxter, is designed to provide an "easier, although perhaps less scientific, method of identifying the orders of British Wild Flowering Plants than is generally found in analytical keys." There is no royal road to botany, and we doubt whether it is any real advantage to the student to sacrifice scientific in favour of superficial characters.

A CORRESPONDING member of the Glasgow Natural History Society, having been lately in Panama, has contributed to a local journal in the latter city an interesting account of the ants of the country. He describes a curious covered way or tubular bridge. In tracing one of these covered ways he found it led over a pretty wide fracture in the rocks, and was carried across in the air in the form of a tubular bridge of half an inch in diameter. It was the scene of busy traffic. There was nearly a foot of unsupported tube from one edge of the cliff to the other.

MR. THWAITES, in his "Enumeration of Ceylon Plants," says that from the large extent of forest land which has been and is now being appropriated to coffee cultivation, there is little doubt that some of the indigenous plants will in time become exceedingly rare, if not altogether extirpated, or exist only in the Botanic Garden, into which as many as possible are being introduced. The obtrusive character, too, of a plant brought to the island less than fifty years since is helping to alter the character of the vegetation up to an elevation of 3,000 feet. This is the *Lantana mixta*, a verbenaceous species introduced from the West Indies, which appears to have found in Ceylon a soil and climate exactly suited to its growth. It now covers thousands of acres with its dense masses of foliage, taking complete possession of land where cultivation has been neglected or abandoned, preventing the growth of any other plants, and even destroying small trees, the tops of which its subscandent stems are able to reach. The fruit of this plant is so acceptable to frugivorous birds of all kinds that, through their instrumentality, it is spreading rapidly, to the complete exclusion of the indigenous vegetation from spots where it becomes established.

METEOROLOGICAL OBSERVATORIES

IN the part of the Quarterly Weather Report of the Meteorological Office just issued, for January—March, 1870, the following information is given with regard to the observatories from which the observations are recorded, accompanied by the illustrations which the courtesy of the committee enables us to reproduce. As correct an idea as possible is thus given of the value of the thermometrical and anemometrical observations published by them, and the local influences which may exert an effect in each case.

VALENCIA.—The observatory is situated close to the shore on the south side of the island, about three miles from the open sea.

The anemograph is on the roof of the house, which is two stories high. Its exposure is fairly good, for although it is situated in a valley, with hills of the height of about 1,000 feet to the south and south-east of it at a distance of three miles, and with a slight hill about 700 feet high distant three-quarters of a mile on the north-west of it, the country towards the other points of the compass is quite open, and the situation for wind is as favourable as can be obtained on that very rugged coast. The only point from which the wind is materially deflected or checked by local influence is the north-west. The house is an ordinary dwelling house of small size.

The thermograph is on its north side, facing due N.W. ½ N., and on the first story. The bulbs of the instruments are at a height of twelve feet above the ground, and about twenty feet above the sea level. The exposure is very good, as there are no buildings or trees in the vicinity to affect the readings.

ARMAGH.—The observatory is on a rising ground close to the town; it is situated in the centre of an ordinary garden and pleasureground, containing trees and shrubs of moderate size.

The anemograph is erected on the roof of the house, and raised seventeen feet above it, and is thoroughly well exposed to all points, excepting that the country about is undulating and fairly well wooded, which has the effect of retarding the motion of the air.

The thermograph screen is erected on the north side of the meteorological observatory; the bulbs are at the distance of four feet from the ground, and about 206 feet above the sea level. The exposure of the screen is good, though there are trees and shrubs about it. However,

VALENCIA

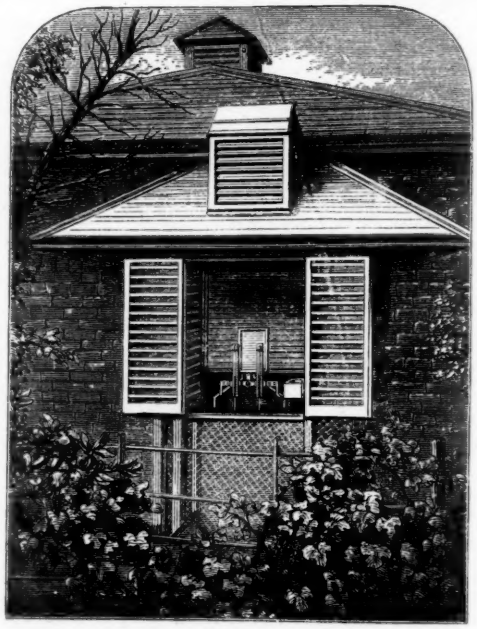
Dr. Robinson has satisfied himself by an independent series of observations that the record taken in the screen gives the true temperature of the place.

GLASGOW.—The instruments are at the astronomical observatory, which is placed on a slight rising ground at the west side of the town, and commands a clear view of the horizon in all directions. It occupies a central position in the valley of the Clyde, which is about 16 miles in breadth at that place. The bounding hills to the north are about 800 feet in height, those towards the south are about 400 ft. high.

The prevailing south-westerly winds sweep along the estuary of the Clyde and reach the observatory without much interruption.

The exposure both of the anemograph and of the thermograph screen is very satisfactory. The former is on the roof of the building, the latter is attached to the north wall of the tower in which the equatoreal is placed. The bulbs are 7 ft. above the ground, and about 190 ft. above sea level.

ABERDEEN.—The observatory is at King's College in Old Aberdeen, and lies on a plane gradually rising from

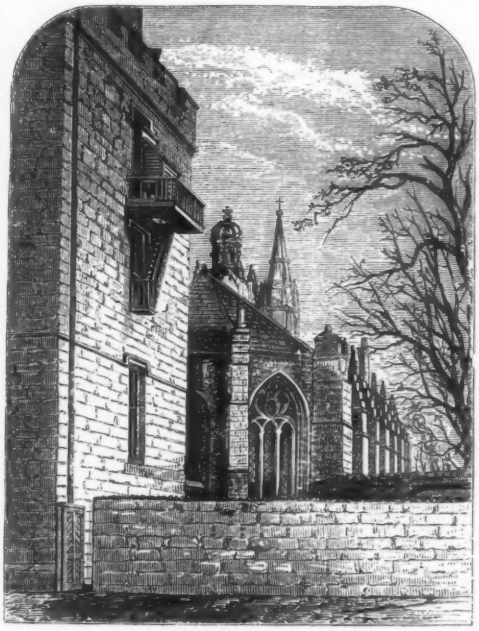


ARMAGH

GLASGOW

the sea, from which it is distant about a mile. There are no irregularities of surface in the vicinity, excepting the two river valleys of the Dee and Don, which are not

great importance. The ground immediately about the buildings is 46 ft. above the mean sea level.

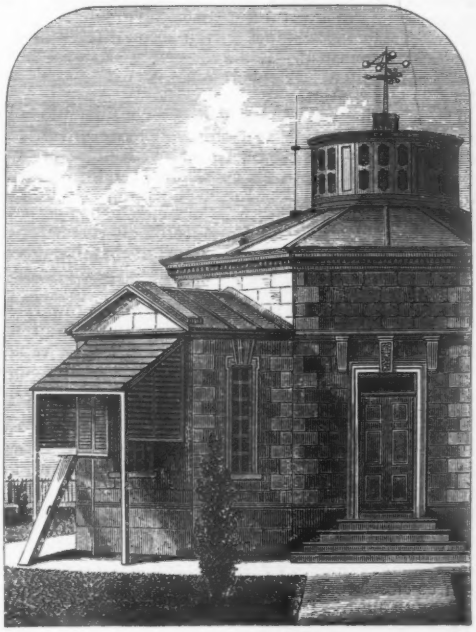


ABERDEEN

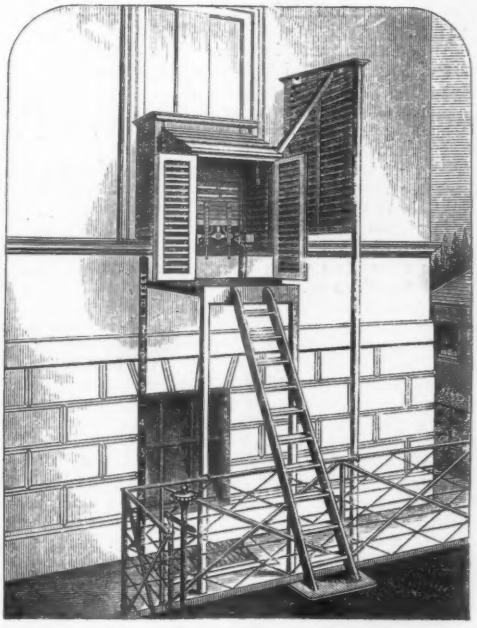
FALMOUTH

The anemograph is erected on the roof of the building, at a height of 72 ft. from the ground. It is well exposed on all sides.

Great difficulties were encountered in obtaining a site for the thermograph screen. The north side of the college



STONYHURST



KEW

is almost entirely occupied by the chapel. One wall of the building in which the physical cabinet and lecture-room are situated also affords a north aspect, but un-

fortunately there are trees growing at a short distance from it, which would entirely check the free circulation of air about the instruments were the screen set up at the usual elevation of about 6 feet above the ground.

Accordingly the window on the second story of the building was selected. It affords a free exposure to the north, but is at a level of 41 ft. above the ground, and about 87 feet above the sea level.

This elevation will of course exert a considerable influence on the thermometrical observations recorded.

FALMOUTH.—The establishment of an observatory at this station was beset with considerable difficulties; the building in which the Royal Cornwall Polytechnic Society holds its meetings was unsuited to the purposes of a meteorological station. Accordingly a tower was erected at the south-east corner of the bowling green, on the top of one of the hills on which the town is built.

The anemograph is on the summit of the tower, well exposed on all sides; but from the fact that the ground in the neighbourhood is uneven, the hill sloping rapidly down to the harbour, it seems probable that the force of the wind is not quite true, especially when it is easterly.

The position of the thermograph screen is far from being quite satisfactory; however, a better exposure could not be obtained. The screen is attached to the north wall of the tower, at an elevation of 11 feet above the ground, and about 200 feet above sea level.

It will be seen that there is the wall of a dwelling house at no great distance to the westward, which may possibly affect the instrument by radiation, and also interfere with the free circulation of the air.

STONYHURST.—The observatory stands in the centre of the college garden, which is on a gentle slope facing S.S.E., 381 feet above sea level. The anemograph stands on a cylindrical roof 12 feet in diameter and 4 feet 5 inches in height. The total height of the cups above the ground is 30 feet.

The country around, including the college grounds, is wooded, but not very thickly so, and the trees are in general small.

The nearest trees whose height could materially influence the anemograph are at a distance of about 200 yards, bearing from N. by W. to N. by E.

The main building of the college is placed at the N.W. of the observatory, at a distance of 193 yards, its angular height above the roof of the observatory being 1° 37′, and bearings from N. by W. to W.N.W.

The nearest hill is the Longridge Fell, whose nearest point is about two miles from the college. It extends from N. by W. to W. by N., and its highest point is 4° 1′.

Pendle Hill is at five and a half miles distance E.N.E.; height 2° 5′. Between these hills the country is very open. To the eastward there are hills at about four miles distance, height about 1°. To the S. and S.W. the land is low.

It will be seen from this that the anemograph is fairly well exposed to the different points of the compass.

The thermograph screen is attached to the north wall of the observatory, the bulbs are at an elevation of 7 ft. above the ground. The exposure is good.

KEW.—The observatory is situated in the old Deer Park at Richmond. It is a small building, which is well exposed to the wind, excepting on the west side, where there is a row of trees distant about 150 yards, which must materially affect the velocity of the wind. The country about is also well wooded.

The anemograph is placed on the dome.

The thermograph screen is attached to the north wall of the observatory within ten feet of the west end of that wall, at a height of ten feet above the ground, and about fifty above sea level. Its exposure is good.

We hope to take another opportunity of reviewing the volume itself.

ON THE RECENT SOLAR ECLIPSE*

(*Continued from page* 233)

II.—POLARISCOPIC OBSERVATIONS

WITH regard to the polarisation experiments, by the kindness of Mr. Spottiswoode I am enabled to show you, in a very clear way, the *raison d'être* of the polariscopic observations made during this and former eclipses; but the polariscopic ground is a wide one, and it is not my intention to cover it to-night.

I have had this arrangement of lamp, reflector and prisms made so that you may see how the polariscope can determine the percentage of reflected light at different angles, and the direction of reflection. Assume this lamp to represent the sun, let this reflector close to the lamp represent a particle near the sun, reflecting light to us, we shall naturally have the light reflected at a much larger angle than if the reflector were close to the screen representing a particle in our own air. Having this idea of the angle of reflection in your minds, and the fact that the larger the angle under these conditions the more the polarisation, if you take this lamp, as I have said, to represent the sun, and this mirror to represent any particle, of whatever kind you choose to imagine, it is clear that in order to get the maximum polariscopic effect from that particle, you must have it so situated that it will reflect light at a considerable angle to the beam coming from this lamp.

Now it is clear that in order to polarise the beam most strongly, I must place the reflector close to our imaginary sun. If I so place it as to represent a particle in our own atmosphere, the angle will be so small that the polarisation of the light will hardly be perceptible.

Here is our sunlight, which we will polarise at as great an angle as we can, by placing the reflector close to the imaginary sun, and send it through this magnificent prism which Mr. Spottiswoode has been good enough to place at our disposal; and in the path of the beam I will place an object so that you determine whether there is polarised light. [Experiment.] You see there is considerable brilliancy in those colours; their brilliancy depending upon the amount of polarisation.

Now if, instead of having our reflector close to our imaginary sun to represent a particle in the sun's atmosphere, we place it near the screen to represent a particle in our own, in which case the angle is extremely small, the brilliancy of the colours will entirely disappear. You see it has disappeared. The colours, as colours, are distinguishable, but their brilliancy has gone.

That is the rationale of the polariscopic observations, which have been made on the occasion of the last eclipse with more elaboration than they ever were before. If we found the corona to be strongly polarised, this was held to be a great argument in favour of the corona being a real solar appendage, an argument strengthened if the polarisation was also found to be radial. At present, however, a great many of the observations that have been made have not been received, and those that have been received are as discordant as those obtained in former eclipses, and therefore my account is an imperfect one, because I have not had an opportunity of discussing all these observations. Indeed, if I had, I should hesitate to give an opinion on the subject. When Mr. Carrington saw that small corona in 1851, and Mr. Gillis saw that small corona in 1858, neither of them traced any polarisation whatever; but when M. Liais saw that large corona in 1868, which was invisible to Mr. Gillis, he in his turn saw an immense amount of polarisation, which led him to believe that the corona was solar, the whole of it, rays and everything included, and that we had an indication of a solar atmosphere two or three times higher than the diameter of the sun; that is, an atmosphere two or three millions of miles in height. This observation is not in accordance with the general conclusions from the drawings I have shown you; and let me add that the assumption of reflection at the sun is not without its difficulties, and that we have not yet traced reflected sunlight, even when the strongest polariscopic effects have been observed.

III.—AIRY'S AND MÄDLER'S CONCLUSIONS AS THE RESULTS OF THE PRE-SPECTROSCOPIC OBSERVATIONS

Before passing to the spectroscopic observations, I will state the conclusions at which the Astronomer Royal and M. Mädler arrived after the observations of 1860 had been gathered together.

The Astronomer Royal, in a lecture delivered before the British Association at Manchester in 1861, stated that the assumption of an atmosphere extending to the moon explained the observation of Plantamour, which could, he thought, be explained

* Lecture delivered at the Royal Institution, Friday, March 17, 1871.

in no other way, and he held also that the polarisation experiments seemed to show the same thing. The Astronomer Royal was content to find the reflection, which so many now insist must be at the sun, taking place somewhere between the earth and moon.

M. Mädler's verdict is in the same direction, and though he does not perhaps express so decided an opinion, he maintains that the atmosphere plays a principal part in the phenomenon; and after detailing experiments to show this, he remarks of the solar and atmospheric portions, "Both cover each other and unite in one phenomenon, so that the corona is a mixed phenomenon."

I shall shortly show you that the spectroscope, leaving the telescope out of consideration, has taught us that this is true; though I shall not be able to show you that it is the whole truth; we are not yet in a position to do that. Mädler concludes his observations by remarking:—"We cannot share the doubts of those who are afraid to surround the sun with too many envelopes; neither do we find anything unnatural in the statement that the sun has as many atmospheres as Saturn has rings; but we gladly admit that we cannot yet say anything positive. We have here a large field of probabilities, and the decision may yet be distant."

We can speak with more certainty now!

IV.—Spectroscopic Observations

a.—Spectrum of the Corona first observed by Tennant, Pogson, and Rayet

We now come to the consideration of those observations in which we are aided by a most powerful and our most recent ally, the spectroscope, first used on the eclipsed sun, as you know, in the eclipse of 1868. You all know that in that year the question of the nature of red flames was for ever settled by M. Janssen, Major Tennant, Captain Herschel, and others, who observed that eclipse in the most admirable manner; but we have nothing to do with the red flames now, we have to do with something outside them.

Now, most of you are under the impression, and it was mine until the day before yesterday, that the only thing we learnt about the corona in the eclipse of 1868, was that its spectrum was a continuous one; and I need not tell anyone in this theatre that the assertion that it was continuous was one that was extremely embarrassing, and implied that we had something non-gaseous outside the red flames, which seemed very improbable to those who know anything about the subject. But some of you will no doubt remember that, besides Major Tennant, who made this observation, we had a French observer, M. Rayet, who gave us a diagram of the spectrum of one of the prominences, and Mr. Pogson, who has now been for some time in India, and is a well-known observer, who gave us, nominally as the spectrum of a prominence, a spectrum with some curious variations from M. Rayet's diagram.

I exhibit a copy of M. Rayet's diagram of the spectrum of a prominence, as he called it. At the bottom is what he considered as the spectrum of the lower portion of the prominence; while in the higher portion, where we get fewer lines, as he considered, is the spectrum of the higher portion of the prominence. The spectrum of the lower portion contains the lines B, D, E, and F, and some other lines, in all nine, while the spectrum of the upper part of the prominence, as he thought it, only contains three lines. It was at first difficult to account for these observations. In the first place, one could not understand the line B being given, because I soon found that the line B was not seen as a bright line in the chromosphere spectrum; it was clearly the line C that was intended. Hence doubt was thrown on the other lines; it seemed as if M. Rayet was wrong about his elongated lines D, E, and F, and probably meant C near D and F. And so it was explained—I am ashamed to say by myself—that there was no particular meaning in these elongated lines, except that the spectrum of the prominence some distance away from the sun was simpler than it was nearer the sun, as happens in all prominences, as we may now determine any day we choose to look at the sun by means of the spectroscope.

Now let us hear Mr. Pogson. He gave a diagram showing five lines in the spectrum of what he thought a prominence, and he writes:—"A faint light was seen (in the spectroscope), scarcely coloured, and certainly free from either dark or bright lines. While wondering at the dreary blank before me, and feeling intensely disappointed, some bright lines came gradually into view, reached a pretty considerable maximum brilliancy, and again faded away. Five of these lines were visible, but two decidedly superior to the rest. . . . . The readings of the two brightest were secured. It struck me as strange that these brightest lines should appear at a part of the spectrum not corresponding to any very conspicuous dark lines in the solar spectrum. . . . . [These lines are a little less refrangible than E.] The third line seen in order of brilliancy must have been either coincident with, or very near the place of the sodium line D, but it was much fainter than the two measured, while the fourth and fifth lines were extremely faint." [They were very faint and DOUBLED, and near F. I have seen F give way to a double line in our hydrogen experiments, though I am not prepared to say this is an explanation of Mr. Pogson's observations.]

The fact that we have here the first observations of the spectrum of the sun's corona is one beyond all doubt; and why M. Rayet and Mr. Pogson thought they were observing prominences when they were observing above them, is explained by a remark made by Captain Tupman, of the Royal Marine Artillery, who acted as jackal to Prof. Harkness, and picked out the brighter spots of the corona for his observation. Prof. Harkness observing the prominence bright lines, said to Captain Tupman, "You have turned the telescope on to a prominence; I want the corona." "No," said Captain Tupman, "I am giving you the corona as well as I can." It was certainly the corona in both cases. Here you see, dimly and darkly, the first outcome of the spectroscope on the nature of the corona; a record as fairly written as anything at the sun can write it; and I am more anxious to lay stress on these observations, since they have lain fallow for two years, and show the importance of observations, not only in extending our knowledge, but in explaining prior observations; and it is an additional reason for never rejecting an observation. What was, however, dim and dark in 1868, shone out brightly in 1869, thanks to the skill of the American observers of the eclipse of that year.

b.—Laboratory Experiments bearing on these Observations

But before I proceed to refer to the admirable observations made in America during this eclipse, I wish to introduce you to some work which was commenced in 1868, and has been done quite independently of eclipses. In a lecture which I delivered here about two years ago, I described to you some of the facts observed by the spectroscope in the bright-line region which had been spectroscopically determined to exist all round the sun, and which, as in it all the various coloured effects are seen in total eclipses, I had named the Chromosphere. It was clear that by the new method of observing this without any eclipse, by partially killing, so to speak, the atmospheric light, we got a percentage only of the phenomenon, as the atmospheric light could only be killed by an amount of dispersion which enfeebled and shortened the chromospheric lines; so that although we could say that an envelope of some 5,000 or 6,000 miles in height existed round the sun, we could not fix this as a maximum limit. Further, when we examined the spectrum of this envelope we got long lines and short lines; and I told how the short lines indicated a low stratum, and how a long line indicated a higher one. To explain this, I will show you an observation made long before the new method was thought of. Even before that time we had abundant evidence of such strata, if we could not determine their nature: we had distinct evidence either of one thing *thinning* out, and then another, or that various substances were situated at different levels, under different conditions; on the first hypothesis, at the extreme outside of the chromosphere the last thing would thin out, and then there would be an end of all things as respects the sun.

I will show you a drawing made by Prof. Schmidt of the eclipse of 1851. I do not wish to call your attention to the strange shape of the large prominence, but to the fact, that as the moon passed over this region we get a thin red band, first along the edge of the dark moon, and after the moon had passed over still further, we see this red layer, *suspended as it were in the chromosphere*, with a white layer below it. This is the explanation of the long and short lines visible in the spectrum of the chromosphere; in the red layer we have hydrogen almost alone; below, its red light was conquered by other light with bright lines in all parts of the spectrum, and we get white light.

Lord Lindsay tells me he has a distinct indication, written by the sun himself, that in one particular part of the chromosphere, as recorded photographically in Spain, there were three such layers. And over and over again we find recorded white light close to the sun, then red alone, or red mixed with yellow, then violet,

and lastly green. And M. Mädler remarks on this very admirably, "The violet band is the link between the prominences and the corona."

Before going further, I will show you the difference in the appearance of what we may term hot hydrogen and cold hydrogen, that is, hydrogen which we drive into different degrees of incandescence by means of the spark. After Dr. Frankland and myself were able to determine that the pressure in these solar regions was small, we came to the conclusion that outside the hot hydrogen there must be some cooler hydrogen, in order that the phenomena we observed, both in the laboratory and in the observatory, should agree.

I have in this tube hydrogen at a certain pressure, and here we have a coil which will enable us to send a spark through it; you see we get a certain amount of redness in that tube, and if you look on one side or above you will see a sort of bluish-greenish light. Now that redness represents the condition of the hydrogen in the region of the sun where Dr. Schmidt gave us that extremely thin red ring, and the combination of the blue and red would give you something very like violet.

But here I have hydrogen under a different condition. In the tube its rareness is not excessive; but in this globe, of which I am about to speak, you have the nearest approach to a vacuum ever obtained through which a spark will pass; and I beg to call your attention to what will now happen. This globe contains the same chemical element prepared at the same time as the chemical element you have in the tube, but you see that, so far as colour goes, we have something perfectly different in this case. Now we send the spark through it. I would beg Prof. Tyndall, if he will be good enough, to observe the spectrum of this hydrogen in this globe. [Prof. Tyndall did so.] You will see that there is one line? [*Prof. Tyndall:* Yes.] And a continuous spectrum? [*Prof. Tyndall:* And a continuous spectrum.] Cool hydrogen gives us only the bright line F, plus a continuous spectrum, and many of you will know the extreme importance of that observation. It accounts for the F line being observed without the C line in 1868 and last year, and also for he continuous spectrum observed in the Indian eclipse.

c.—The American Eclipse

When we come from the Indian to the American eclipse with the considerations to which I have drawn your attention, namely, the existence of these different layers due to the different elements and conditions of the same element thinning out, we shall see the extreme importance of the American observations, for they establish the fact that outside the hydrogen layer there was a layer giving only a line in the green, the line which Rayet and Pogson had observed associated with the hydrogen spectrum and the spectrum of the yellow substance. Here obviously we have, I think, merely an indication of another substance thinning out, in spite of the extraordinary suggestion which was put forward that the corona was nothing but a *permanent solar aurora.*

I need hardly tell you that the idea of a permanent aurora anywhere was startling, and that of a permanent solar aurora more startling still; but what I claim is, that during last year's observations we made this very startling idea into a most beautiful fact, namely, that this outer layer of the chromosphere is in all probability nothing more nor less than an indication of an element lighter than hydrogen, although this is not yet absolutely established, for the line is coincident with one of the lines in the spectrum of iron.

d.—The layers increase very rapidly in Density. Reproduction of the Coloured Phenomena

Dr. Frankland and myself were early drawn to consider the solar nature of the large coronas, to which I have called your attention, as extremely questionable, even on the supposition of cool hydrogen, because we did not see how, with its temperature and pressure, it could extend very far: and an experiment which I have to make here will probably make that clearer.

We have in these glass vessels hydrogen a little more brilliant now the spark passes through it than that you saw in the globe, because I have been compelled to mix with it a certain amount of mercury vapour. Below, we have at the present moment sodium vapour being generated from metallic sodium in one tube, and mercury vapour in the other. I hope, if the experiment succeeds, you will see that a good many of the coloured phenomena seen in the chromosphere during eclipses may be easily reproduced by such experiments as this; and not only the coloured phenomena but *the increase of brilliancy* accompanied by changes of colour recorded. You can now all see the yellow tinge at the bottom of one tube, and the green tinge at the bottom of the other; and if there were time to continue this experiment by increasing the density of the vapours now associated with the hydrogen, I could make the bottom portion of each tube where the vapours are densest shine out almost like the sun, while the cool hydrogen at the top would remain not more brilliant than it is at present. We should have as it were a section of the chromosphere.

V.—Conclusion

I will proceed now, if you will allow me, to some of the general results obtained during the last eclipse.

I think that, although the work has been very unfortunately interrupted, still the result has been most satisfactory. By putting together observations here and observations there, I consider our knowledge of the sun is enormously greater than it was a few months ago. For instance, we are enabled to understand the long-neglected observation of Rayet, and the equally long-neglected observation of Pogson; and we know that outside the hydrogen there is, in all probability, a new element existing in a state of almost infinite tenuity. And we are sure of the existence of cool hydrogen above the hot hydrogen, a fact which seemed to be negatived by the eclipse of 1869.

I think if we had merely determined that there was this cool hydrogen, all our labour would not have been in vain, as it shows the rapid reduction of temperature. But there is more behind. I told you that M. Mädler, in summing up the observations made up to 1860, came to the conclusion that part of the corona was certainly solar, and that whether the outer portions were or were not solar, was a matter of doubt. I do not say that we have settled that absolutely, but we have firm evidence that *some* of the light of the corona is due to reflexion between the earth and the moon. The outer corona was observed to have a rosy tinge over the prominences, and the spectrum of the prominences was detected many minutes above them, as well as on the dark moon. It could not have got this colour *at the sun*, for its intrinsic colour is green, and the red light of the hydrogen supplied at the sun is abolished altogether, is absorbed, and can only reach the corona *at the sun*, so to speak, as dark light.

It is a great fact that we are sure, as far as observation can make us sure, that there is a glare round the hydrogen which gives us the spectrum of hot hydrogen on the corona, *where we know that hot hydrogen does not exist.* Assume the hot hydrogen which gives us the red light to be only two minutes high, the spectroscope has picked it up eight minutes from the sun! The region of cool hydrogen is exaggerated in the same way. We get it where there is no indication of the cool hydrogen existing. And then with regard to the element which gives us the line of the green, we get that twenty minutes or twenty-five minutes away from the sun. Well, no man who knows anything about the matter will affirm that it is certain that the element exists at that distance from the sun.

Therefore I think we have absolutely established the fact that as the sun—the uneclipsed sun—gives us a glare round it, so each layer of the chromosphere gives us a glare round it. That is exactly what was to be expected, and that it is true is proved by the observation—a most important observation made in Spain—that the air, the cloud, ever between us and the dark moon, gives us the same spectrum that we get from the prominences themselves.

Given, however, the layers and elements in the chromosphere extended as far as you will, and apparently increased or not by reflection *not at the sun*, we have still to account for rays, rifts, and the like. If anyone will explain either Mr. Brothers's photograph or Mr. Gilman's picture of the eclipse of 1869, containing those dark bands starting from the moon and fading away into space, and the bright variously-coloured rays between them, on any solar theory, he will render great service to science. But in the meantime I must fall back upon M. Mädler's opinion of 1860, with the addition to it that I have stated that we have found, at all events, that *some* of the doubtful light is non-solar; we have turned the opinion into a fact.

Bear in mind that close to the sun you have a white layer composed of vapours of many substances, including all the outer ones; outside this is a yellow region; above that a region of hydrogen, incandescent and red at the base, cooler, and therefore blue, higher up, the red and blue commingling and giving us violet; and then another element thinning out and giving us green. Take these colours in connection with those which are thrown on our landscapes or on the sea during eclipses, each region being lit up in turns with varying, more or less mono-

chromatic light, and that light of the very colour composing the various layers, each layer being, as I have shown, so much brighter than the outer ones that its light predominates over them. Is it too much to suggest to those who may be anxious to attempt to elucidate this subject, that probably if they would consider all the conditions of the problem presented by that great screen, the moon, allowing each of these layers by turn to throw its light earthwards, the inequalities of the edge of the *globular* moon allowing here light to pass from a richer region, here stopping light from even the dimmer ones, they would be able to explain the rays, their colours, variations, apparent twistings, and change of side? I do not hesitate to ask this question, because it is a difficult one to answer, since the whole question is one of enormous difficulty. But difficult though it be, I trust I have shown you that we are on the right track, and that in spite of our bad weather, the observations made by the English and American Government Eclipse Expedition of 1870 have largely increased our knowledge.

With increase of knowledge generally comes a necessity for changing the nomenclature belonging to a time when it was imperfect. The researches to which I have drawn your attention form no exception to this rule. A few years ago our science was satisfied with the terms prominences, sierra, and corona, to represent the phenomena I have brought before you, the nature of both being absolutely unknown, as is indicated by the fact that the term *sierra* was employed, and aptly so, when it was imagined the prominences might be solar mountains! We now know many of the constituent materials of these strange things; we know that we are dealing with the exterior portion of the solar atmosphere, and a large knowledge of solar meteorology is already acquired, which shows us the whole mechanism of these prominences. But we also know that part of the corona is not at the sun at all. Hence the terms *leucosphere* and *halo* have been suggested to designate in the one case the regions where the general radiation, owing to a reduced pressure and temperature, is no longer subordinate to the selective radiation, and in the other, that part of the corona which is non-solar. Neither of these terms is apt, nor is either necessary. All purposes will be served if the term corona be retained as a name for the exterior region, including the rays, rifts, and the like, about which doubt still exists, though it is now *proved* that some part is *non-solar*, while for the undoubted solar portion the term Chromosphere—the bright-line region—as it was defined in this theatre now two years ago, exactly expresses its characteristic features, and differentiates it from the photosphere and the associated portion of the solar atmosphere.

Here my discourse would end, if it were not incumbent on me to state how grateful I feel to Her Majesty's Government for giving us the opportunity of going to the eclipse; to place on record the pleasure we all felt in being so closely associated in our work with the distinguished American astronomers who from first to last aided us greatly; and to express our great gratitude to all sorts of new friends whom we found wherever we went, and who welcomed us as if they had known us from our childhood.

J. NORMAN LOCKYER

ON THE DISTRIBUTION OF TEMPERATURE IN THE NORTH ATLANTIC *

AT the request of the Council of the Scottish Meteorological Society, I beg to bring before you a sketch of the more recent results of investigations into the causes of the abnormal climate of the surface of a great portion of the North Atlantic Ocean, and of the lands which form its north-eastern borders; and especially the results of the deep-sea exploring expeditions of the last three years, in which I have taken a part, so far as they bear upon this point.

In a recent valuable report on the Gulf Stream in the "Geographische Mittheilungen," of last year, Dr. Petermann severely and, I think, too justly, reflected upon us students of ocean temperatures for giving ourselves up to wild and gratuitous speculation. I wish, if possible, on the present occasion, to avoid all risk of such impeachment, by limiting our inquiry rigidly for the few minutes I have at my disposal to the present condition of our knowledge of facts, and to such deductions from these as may be fairly considered proved.

* Address delivered to the Meteorological Society of Scotland at the General Meeting of the Society, July 5.

Let us then first inquire for a moment what the phenomena are which we are called upon to correlate and to explain. There is no dispute about these facts, and a glance at the chart will at once recall them to your recollection. In the first place, the lines of equal mean annual temperature, instead of showing any tendency to coincide with the parallels of latitude, run up into the North Atlantic and into the North Sea, in the form of a series of long loops. This diversion of the isothermal lines from their normal direction is admittedly caused by surface ocean-currents conveying the warm tropical water towards the polar regions, whence there is a constant counter-flow of cold water beneath to supply its place. This phenomenon is not confined to the North Atlantic. A corresponding series of loops, though not so well defined, passes southwards along the east coast of South America, and a very marked series occupies the north-eastern angle of the Pacific, off the Aleutian Islands and the coast of California. The temperature of the land is not affected directly by the temperature of the sea in its immediate neighbourhood, but by the temperature of the prevailing wind, which is determined by that of the sea. Setting aside the still more important point of the equalisation of summer and winter temperature, the mean annual temperature of Bergen, lat. 60° 24′ N., subject to the ameliorating influence of the south-west wind blowing over the temperate water of the North Atlantic, is 6·7° C. while that of Tobolsk, lat. 58° 13′, is −2·4° C.

But the temperature of the North Atlantic is not only raised greatly above that of places on the same parallel of latitude having a continental climate by this interchange of tropical and polar water, but it is greatly higher than that of places apparently similarly circumstanced as to a general interchange of water in the Southern Hemisphere. Thus, the mean annual temperature of the Faroe Islands, lat. 62° 2′ N. is 7·1° C. nearly equal to that of the Falkland Islands, lat. 52° S., which is 8·2° C., and the temperature of Dublin, lat. 53° 21′ N., is 9·6° C., while that of Port Famine, lat. 53° 8′ S., is 5·3° C. Again the high temperature of the North Atlantic is not equally distributed, but is very marked in its special determination to the north-east coasts. Thus, the mean annual temperature of Halifax, lat. 44° 39′, is 6·2° C., while that of Dublin, lat. 53° 21′ is 9·6 C., and the temperature of Boston (Mass.) lat. 42° 21′ is exactly the same as that of Dublin.

We thus arrive at the well-known general result, that the temperature of the sea bathing the north-east shores of the North Atlantic is greatly raised above its normal point by currents involving an interchange of tropical and polar water; and that the lands bordering on the North Atlantic participate in this amelioration of climate by the heat imparted by the water to their prevailing winds.

We shall now examine this distribution of ocean temperature a little more minutely. During the last many years a prodigious amount of data have been accumulating with reference to the detailed distribution of heat on the surface of the North Atlantic basin, and last year M. Petermann, of Gotha, published in his "Geographische Mittheilungen" a series of invaluable temperature charts embodying the results of the reduction of upwards of 100,000 observations derived mainly from the following sources:—

1st. From the wind and current charts of Lieut. Maury, embodying about 30,000 distinct temperature observations.

2nd. From 50,000 observations made by Dutch sea captains and published by the Government of the Netherlands.

3rd. From the journal of the Cunard steamers between Liverpool and New York, and of the steamers of the Montreal Company between Glasgow and Belleisle.

4th. From the data collected by our excellent secretary, Mr. Buchan, with regard to the temperature of the coast of Scotland.

5th. From the publications of the Norwegian Institute on sea temperatures between Norway, Scotland, and Iceland.

6th. From the data furnished by the Danish Rear-admiral Irminger on sea temperatures between Denmark and the Danish settlements in Greenland.

7th. From the observations made by Lord Dufferin on board his yacht *Foam* between Scotland, Iceland, Spitzbergen, and Norway.

And finally from the recent observations collected by the English, German, Swedish, and Russian expeditions to the Arctic Regions and towards the North Pole.

Dr. Petermann has devoted the special attention of a great part of his life to this question, and the accuracy of his results in every detail is beyond the shadow of a doubt. Every curve of equal temperature, whether for the summer, for the winter, or for the

whole year, instantly declares itself as one of a system of curves which are referred to the Strait of Florida as the source of heat, and the warm water may be traced (and this is not begging the question, for the temperature is got by dipping the thermometer in the water), in a continuous stream, indicated where its movement can no longer be observed by its form, fanning out from the neighbourhood of the Strait across the Atlantic, skirting the coasts of France, Britain, and Scandinavia, rounding the North Cape, and passing the White Sea and the Sea of Kari, bathing the western shores of Novaja Semla and Spitzbergen, and finally coursing round the coast of Siberia, a trace of it still remaining to try to find its way through the narrow and shallow Behring's Strait into the North Pacific. Now it seems to me that if we had these observations alone, which are merely detailed and careful corroborations of many previous ones, and could depend upon them, without even having any clue to their *rationale*, we should be forced to admit that whatever might be the amount and distribution of heat derived from a general oceanic circulation, whether produced by the prevailing winds of the region, by convection, by unequal barometric pressure, by tropical heat, or by arctic cold, there is besides this some other source of heat at the point referred to by these curves sufficiently powerful to mask all the rest, and, broadly speaking, to produce of itself all the perceptible deviations of the isotherms from their normal course.

But we have no difficulty in accounting for this source of heat. As is well-known, about the equator, the north-east and south-east trade winds reduced to meridional directions by the eastward frictional impulse of the earth's rotation, drive before them a magnificent surface current of hot water, the equatorial current, 4,000 miles long and 450 miles broad, at an average rate of thirty miles a-day. This current splits upon Cape St. Roque, and one portion trends southwards to deflect the isotherms of 21° 15·5°, 10°, and 4·5° C. into loops, thus carrying a scrap of comfort towards the Falklands and Cape Horn. While the remainder, "having made the circuit of the Gulf of Mexico, issues through the Straits of Florida, clinging in shore round Cape Florida, whence it issues as the Gulf Stream, in a majestic current upwards of 30 miles broad, 2,200 feet deep, with an average velocity of 4 miles an hour, and a temperature of 86° Fahr." (Herschel.)

I need scarcely follow the course of the Gulf Stream in detail, it is generally so well known. After leaving the Strait of Florida, it strikes in a north-easterly direction conformable generally to the easterly impulse given by its excess of diurnal rotation, towards the coast of Northern Europe. About 42° N. a large portion of it, still maintaining the high surface temperature of 24° C., turns eastward and southward, and, eddying round the Sargasso Sea, fuses with the northern edge of the equatorial current, and rejoins the main circulation. The main body, however, moves northwards. Mr. Croll, in a very suggestive paper in the *Philosophical Magazine* on Ocean Currents, estimates the Gulf Stream as equal to a stream of water fifty miles broad and 1,000 feet deep, flowing at a rate of four miles an hour, with a mean temperature of 18° C. I see no reason whatever to believe this calculation to be excessive, and it gives a graphic idea of the forces at work.

The North Atlantic and the Arctic Seas form together a basin closed to the northward, for there is practically no passage for a body of water through Behring's Strait. Into the corner of this basin, as if it were a bath, with a north-easterly direction given to it, as if the supply pipe of the bath were turned so as to give the hot water a definite impulse, this enormous flood is poured day and night, winter and summer; almost appalling in its volume and the continuity of its warmth, and its blueness, and brilliant transparency *in sæcula sæculorum!*

The hot water pours, not entirely from the Strait of Florida, but partly from the Strait and partly in a more diffused current outside the islands, with a decided, though slight, north-easterly impulse on account of its great initial velocity. The North Atlantic is with the Arctic Sea a *cul-de-sac*. When this basin is full—and not till then—overcoming its northern impulse, the water tends southwards in the southern eddy, so that there is a certain tendency for the hot water to accumulate in the northern basin. It is to this tendency, produced by the absence of a free outlet to the Arctic Sea, that I would be inclined to attribute the special excess of the warmth of the north-eastern shores of the North Atlantic.

When ascertaining with the utmost care and with the most trustworthy instruments, by serial soundings, the temperature of the area surveyed by the *Porcupine* in 1869, we found at a depth of 2,435 fathoms in the Bay of Biscay, that down to 50 fathoms the temperature of the sea was greatly affected by direct solar radiation; from 100 to 900 fathoms the temperature gradually fell from 10° C. to 4° C., and from 900 fathoms to 2,435 the fall of temperature was almost imperceptibly gradual from 4° to 2°·5 C.

The comparatively high temperature from 100 fathoms to 900 fathoms I am certainly inclined to attribute to the northern accumulation of the water of the Gulf Stream. The radiant heat derived directly from the sun must of course be regarded as a constant quantity superadded to the original temperature of the water derived from other sources. Taking this into account, the surface temperatures in what we were in the habit of calling the "warm area" coincided precisely with Petermann's curves indicating the northward path of the Gulf Stream.

It is scarcely necessary to say that for every unit of water which enters the basin of the North Atlantic, an equivalent must return. From its low velocity, the Arctic return current or indraught will doubtless tend slightly to a westerly direction, and the higher specific gravity of the cold water may probably even more powerfully lead it into the deepest channels; or possibly the two causes may combine, and in the course of ages the currents may tend to hollow out deep south-westerly grooves. At all events, the main Arctic return currents are very visible on the chart taking that direction, indicated by marked deflections of the isothermal lines. The most marked is the Labrador current, which passes down inside the Gulf Stream along the coasts of Carolina and New Jersey, meeting it in the strange, abrupt "cold wall," dipping under it as it issues from the Gulf, coming to the surface again on the other side, and a portion of it actually passing under the Gulf Stream as a cold counter-current into the deeper part of the Gulf of Mexico.

Fifty or sixty miles out from the west coast of Scotland, I believe the Gulf Stream forms another through a very mitigated "cold wall." In 1868 Dr. Carpenter and I investigated a very remarkable cold indraught into the channel between Shetland and Faroe. In a lecture on deep-sea climates, which was published in NATURE, in July last, I stated my belief that the current was entirely banked up in the Faroe channel by the Gulf Stream passing its gorge.

Since that time I have been led to suspect that a part of the Arctic water oozes down the Scottish coast much mixed, and sufficiently shallow to be affected throughout by solar radiation. About sixty or seventy miles from shore the isothermal lines have a slight but uniform deflection. Within that line types characteristic of the Scandinavian fauna are numerous, and in the course of many years' use of the towing net, I have never met with any of the Gulf Stream pteropods, or of the lovely Polycystinæ and Acanthometrinæ, which absolutely swarm beyond that limit. The differences in mean temperature between the east and west coasts of Scotland, amounting to between 1° and 2° Fahr., is also somewhat less than might have been expected.

There is another point which is worthy of consideration. It is often said that about the latitude 45° N. the Gulf Stream thins out and disappears. The course of a warm current is traced farther on the maps, even to the coast of Norway and the North Cape, but this north-easterly extension is called the Gulf Stream drift, and is supposed to be a surface flow caused by the prevailing S.W. anti-trades. There seem to me to be several arguments against this view. The surface of the sea, at all events between 40° and 55° N., has a mean temperature higher than that of the air, and that could scarcely be the case unless there were a constant supply, independent of the wind, of water from a warmer source; and any question is, to my mind, entirely set at rest by our establishment of the mass of warm water moving to the north-eastward, whose curves of excess of temperature, so far as they have as yet been ascertained, correspond entirely with those of the Gulf Stream.

I cannot at present enter at any length into the very fundamental question which has lately given rise to so much discussion, whether the Gulf Stream is actually the agent in conveying heat to the North Atlantic and ameliorating the climate of its north-eastern shores, or whether these results are not rather produced by a "general oceanic circulation."

As, however, I am frequently quoted by my friend and colleague in much scientific work, Dr. Carpenter, as holding an opinion different from his, and as my present remarks place my views beyond doubt, it may be well to give a reason for my want of faith. Dr. Carpenter's view, if I understand him rightly, is that there is a great general convective circulation in the ocean, on the principle of a hot-water heating apparatus, and that the Gulf

Stream is only a modified and partial cause of this general circulation. Now in the first place, as I have already said, it seems to me that the distribution of warm water in the North Atlantic has been traced to its source, and all the general phenomena of the Gulf Stream, its origin, its course, its extension, and its depth at certain points, have been *proved* by the careful observations of many years, which I see no reason whatever to doubt. The constant impulse of the trade wind drives a broad current of equatorial water against the American coast. A great part of this current is observed to turn northwards through the Strait and round the islands, and to pour an eternal flood of hot water in a certain direction, under known laws, into the closed basin of the North Atlantic, and as a natural consequence the temperature is very considerably raised.

We are undoubtedly most deeply indebted to Dr. Carpenter for the forcible way in which he has brought forward the arguments on the other side; and, after carefully considering everything, I am thoroughly willing, with Sir John Herschel, to cede that "there is no refusing to admit that an oceanic circulation of some sort must arise from mere heat, cold, and evaporation as *veræ causæ*;" and that "henceforward the question of ocean currents will have to be studied under a twofold point of view;" but my strong conviction is that if the sagacious philosopher whose loss we now deplore, had been spared so to study it, he would have only been strengthened in his verdict of 1861 as to the Gulf Stream, that there can be no "possible ground for doubting that it owes its origin entirely to the trade-winds." Dr. Carpenter attributes the general oceanic circulation, of which he regards the Gulf Stream as only a modified case, to tropical heat and evaporation, and arctic cold, possibly aided by differences of barometric pressures; or to convection pure and simple, as illustrated in his experiments before the Royal Institution and the Geographical Society. Now what we expect of Dr. Carpenter before we are called upon to accept to the full his magnificent generalisation, is a calculation and demonstration of the amount of the effect of the causes upon which he depends acting under the special circumstances. We must remember that heat is received by the ocean at the surface only, and that owing to cold indraughts all over the globe, so far as we know the temperature falls the deeper we go; that all our observations tend to show that the temperature of the sea is only influenced by direct solar radiation to any amount to the depth of fifty fathoms, so that all currents depending upon difference between equatorial and polar temperatures must be produced and propagated in a film of water about the depth of the height of St. Paul's and 6,000 miles long. The black line bounding that chart represents pretty nearly the depth of the ocean, and even where the whole of the water supposed to be involved in the movement, it would be difficult to imagine a perceptible current to be produced in so thin and wide a sheet by such feeble cause. It would be impossible to indicate by the finest hair line the tenuity of the film which is actually affected by the direct rays of the sun. How differences in barometric pressure can produce constant currents I do not see. Rapid fluctuations in pressure in places within a short distance of one another will doubtless produce readjustment by a wave motion; but constant differences of pressure will simply produce constant differences of level and no currents. Varying pressures at very distant points cannot possibly produce a constant current. I freely admit that I am quite incapable of undertaking the investigations which might lead to the estimation of the relative or actual importance of these causes of currents. I have several times put the question to specialists in such physical inquiries, but they have always said that it was a matter of the greatest difficulty, but that their impression was that the effects would be infinitesimal.

I fear then that, in opposition to the views of my distinguished colleague, I must repeat that I have seen as yet no reason to modify the opinion which I have consistently held, that the remarkable conditions of climate on the coasts of Northern Europe are due in a broad sense solely to the Gulf Stream; that is to say, that while it would be madness to deny that in a great body of water at different temperatures, under varying barometric pressures, and subject to the surface drift of variable winds, currents of all kinds variable and more or less permanent must be set up, yet the influence of the great current which we call the Gulf Stream, the reflux in fact of the great equatorial current, is so paramount as to reduce all other causes to utter insignificance.

WYVILLE THOMSON

PHYSIOLOGY

The Mouse's Ear as an Organ of Sensation*

DR. SCHOBL, of Prague, who lately published a remarkable paper on the wing of the bat, has made similar researches on the ear of the white mouse, with very interesting and surprising results (in "Schultze's Archiv," vol. vii. p. 260.) The first thing which struck Dr. Schöbl was the immense and "fabulous" richness of the ear in nerves. Even the bat's wing is but poorly supplied in comparison. The outer ear was carefully divided horizontally through the middle of the cartilage into two laminæ, each of which was found to be equally supplied with nerves, and was then examined by removing the epidermis and the Malpighian layer of the skin. In each of these laminæ were discovered three distinct strata of nerves, which are thus described: The first or lowest stratum lies immediately upon the cartilage; it consists of the largest trunks which enter the ear, 5 to 7 in number, and their next branches, varying from ·074 mm. to ·028 mm. in diameter. The mode of division of these trunks is mainly dichotomous, but they are connected by several different kinds of anastomoses; as, for instance, by decussation of two adjacent trunks, by transverse or oblique connecting branches, by plexuses, by loops, &c.; while branches also perforate the cartilage, and bring the nerves of the two halves of the ear into connection. The general distribution agrees with that of the larger blood-vessels. The second stratum lies immediately over the first, and is connected with it by a multitude of small branches, and by a fine marginal plexus at the outer border of the ear, which may be regarded as common to both. The diameter of its nerves is from ·0185 mm. to ·0098 mm.; it lies immediately under the capillary vascular network of the skin, and has a generally reticulated arrangement, forming plexuses of very various shapes. The third stratum of nerves, developed out of the very finest twigs of the second, lies at the level of the capillary network; it is composed of branches ·0098 mm. to ·0037 mm. in thickness, which (like those of the other strata) contain medullated nerve-fibres. It forms an extremely delicate network, like the second layer, but its finest branches may terminate in two ways. Some of them, each containing two to four medullated fibres, run directly to the hair follicles, and form a nervous ring round the shaft of the hair, terminating below the follicle in a nervous knot. Others, again, consisting of not more than two medullated fibres, bend towards the surface where the fibres lose their double outline, and form, immediately under the Malpighian layer of the skin, a fine terminal network of pale fibres, which is the fourth and ultimate stratum of nervous structures. The terminal "knots" or corpuscles, and the nervous rings, are inseparably connected with hairs and their sebaceous glands, so that through the whole of the external ear no hair can be found without this nervous apparatus, and *vice versâ*. The connection of the hair follicle with the nerve termination is as follows:—Under the bulk of the hair in each follicle is a more or less conical prolongation, composed of distinct nucleated cells, which run vertically downwards, and is enclosed within the limiting membrane of the follicle. The nervous twig which, as has been said, runs to each hair follicle from the third stratum of nerves, makes several turns round the shaft of the hair, and from the ring thus formed two to four nerve-fibres run vertically downwards to the prolongation of the follicle, immediately beneath which they form a knot. These knots are almost always spherical, sometimes oval, and about ·015 mm. in diameter. In each square millimetre of the marginal part of the ear there are about 90 such bodies, and near the base perhaps 20, so that the average number may be 30. Calculating from the average size of the ear of a common mouse, it is then found that there are on the average 3,000 nerve terminations on each of its surfaces, making 6,000 on each ear, or 12,000 altogether. The function of this elaborate arrangement would seem to be, like that in the wing of the bat, to supply by means of a very refined sense of touch, the want of vision to these subterranean animals.

SCIENTIFIC SERIALS

PART ii. of the *Zeitschrift für Ethnologie* contains No. 6 of Dr. Hartmann's "Studies of the History of Domestic Animals," on the yak or grunting ox (*Bos grunniens*) living wild at immense altitudes in the mountains of Central Asia north of the Himalaya, and largely used in a domesticated state in Mongolia and

* From the "Quarterly Journal of Microscopical Science" for July.

Siberia. The cross between the yak and common ox has the advantage of thriving in a milder climate than that of the mountainous region of the yak. Dr. Hartmann also continues (No. 2) his summary of the available information as to "Lake Dwellings," here discussing their cultivation and preparation of grain and other vegetables. He reaffirms the usual conclusion that the cultivated plants of the lake dwellers of Central Europe indicate connection with the Mediterranean and even Africa. Perhaps the most remarkable point in the paper is the comparison of their large earthen jars for store corn, and their stone grain rubbers for mealing it, with similar jars and grain rubbers in modern Africa.—Prof. Meinicke's "Remarks on Wallace's Views as to the Population of the Indian Islands" are written in strong opposition to the English naturalist's theory as to the ethnological relations of Malays, Polynesians, and Papuans. With regard to Mr. Wallace's argument from contrast of the Malay character with the Papuan as proving difference of race, Prof. Meinicke argues that the Malay's courtesy and reserve may not be a race-character at all, but an effect of conversion to Mohammedanism; while revenge and bloodthirstiness belong to some Papuans as much as to Malays. In opposition to Mr. Wallace's view of Malays and Papuans being two distinct races, and of the Moluccas being largely populated by their intermixture, Prof. Meinicke claims the natives of the Moluccas as intermediate varieties forming a link of connection between the extreme Malay and Papuan types. As to the relation between Malays and Polynesians, Prof. Meinicke maintains the old and generally received view of an ethnological connection between them.—It is good evidence of the activity with which the science of man is now being pursued that Dr. W. Koner's useful bibliography of Anthropology, Ethnology, and Prehistoric Archæology for 1869-70 extends to twenty pages of the journal.—Dr. Bastian's review of Darwin's "Descent of Man," expressing high admiration for its hypothetically-arranged evidence as a contribution to science, protests against the exaggeration of Darwinism, or rather, the return to Lamarckism prevalent among too impetuous followers of the development theory.

In the July number of the *Geological Magazine* (No. 85) the editor, Mr. Woodward, publishes a most interesting summary of the evidence extant as to the existence of limbs in the Trilobites, with a discussion of the significance of a remarkable specimen of *Asaphus*, lately described by Mr. Billings in the *Quarterly Journal of the Geological Society*. From a personal examination of the specimen, Prof. Dana was led to declare that the objects described by Mr. Billings as legs were merely calcified portions of the ventral integument destined to support branchial appendages. Mr. Woodward shows, and we think satisfactorily, that Prof. Dana is in error here. This valuable paper is illustrated with a plate contrasting the lower surface of Mr. Billings's Trilobite with that of the Norway lobster.—Mr. Hull contributes some observations on the general relations of the drift deposits of Ireland to those of Great Britain, in which the author confirms and extends the views adopted by Prof. Harkness as to the correlation of the Irish drift deposits with those of Britain, and the accordance of the whole with the principles laid down by Mr. Searles V. Wood, jun. A tabular statement of the phenomena of the three stages of the drift period in Britain concludes this paper.—From Mr. G. A. Lebour we have a note on the submergence of Is in western Brittany, in which, after referring to a Breton tradition that a town named Is was submerged in the Bay of Douarneney some fifteen hundred years ago, he adduces certain evidence to show that a gradual depression is taking place along this coast. He notices a submerged forest in the small Bay de la Forêt.—Mr. Mackintosh continues his paper on the drifts of the west and south borders of the Lake district; Mr. A. G. Cameron describes the recently-discovered caverns at Stainton in Furness; and Mr. J. E. Taylor discusses the relation of the Red to the Norwich Crags.

The first part of the fourteenth volume of the *Atti della Società Italiana di Scienze Naturali*, published in April of the present year, contains only three papers, more than one-third of its pages being occupied by the annual report, list of members, &c. The papers are a description of a new species of Dalmatian shell, by MM. A. and G. B. Villa, to which the authors give the name of *Clausilia de Cattaniæ*; a long memoir on rennet and caseification, by M. C. Besana, and a short notice by Dr. C. Marinoni, of some new prehistoric remains collected in Lombardy.

SOCIETIES AND ACADEMIES

LONDON

Geologists' Association, July 7.—Prof. Morris, vice-president, in the chair. Mr. J. R. Pattison read a paper "On the Upper Limits of the Devonian System." The author did not wish to reopen the controversy which had taken place between the late Prof. Jukes and the supporters of the classification of the older geologists, but simply to lay before the Association a few facts as a prelude to a more complete paper which he hoped to bring forward during the next session. Mr. Pattison referred at some length to the fauna of the continental Devonian rocks, and strongly opposed the view recently put forward, that the Petherwin series is Lower Devonian and not Upper as generally supposed. He quite agreed with the older geologists in their classification, and concluded by recommending the sections exposed in North Devon to the attention of young geologists.—After some remarks by Prof. Tennant, Mr. Henry Woodward, and Mr. Lobley, Prof. Morris described the distribution of the Devonian rocks throughout Europe, and remarked on the absence of vertebrate remains in the Devonian rocks of the South of England, in which corals and brachiopods abound, and the abundance of vertebrate remains in the Devonians or Old Red sandstones of Scotland, in which neither corals nor brachiopods have been detected. In the province of Oranburg, in Russia, however, the Devonian rocks contain both a vertebrate and a molluscan fauna.—A note "On a New Section of the Upper Bed of the London Clay," by Mr. Caleb Evans, drew the attention of the Association to an interesting exposure of a very fossiliferous bed of the London clay at Child's Hill, Hampstead. From an inconsiderable excavation at this place, Mr. Evans had collected in a short time twenty-three species, chiefly gasteropoda, in a fine state of preservation. This bed Mr. Evans considers to be the uppermost bed of the London clay, and immediately underlying the Bagshot sands, which form the summit of Hampstead Heath.

MAURITIUS

Meteorological Society, April 28.—The Honorable Colville Barclay, vice-president, in the chair.—The following letters and publications were laid upon the table:—1. A letter from Mr. James Duncan, Government Surveyor, forwarding a copy of observations taken at the Survey Camp, Vacoas, during the month of March last, at about 1,850 feet above the sea-level. 2. A letter from Mr. G. Jenner, Rodrigues, forwarding observations taken there in December, January, February, and March last. 3. From Mr. F. Timperley, Pamplemousses, giving a description of a meteor seen by him on the 22nd March. 4. Queensland Observations for October, November, and December 1870, by Mr. Edmund MacDonnell. 5. Singapore Observations for January 1871, by Dr. H. L. Randell.—"On the Converging of the Wind in Cyclones." The Secretary read the following letter addressed to him on the above subject by Captain Douglas Wales, Harbour Master:—"Some remarks of yours respecting the uncertainty of the real position of the centre of a cyclone set me thinking, and I send you a few ideas on the subject, which, as a sailor, I think worthy the serious attention of seamen, and the correctness of which they may put to the test of experience, whenever they have opportunities of doing so. Allow me to premise that I have no intention of dogmatising. I believe our knowledge of the cause of these fearful tempests, of their origin, their progress in this or that direction, their rate of progression, their recurving, the reasons of those recurvings, and their ultimate dispersion, to be still in its infancy. No doubt, the knowledge already acquired has saved many a good ship from becoming entangled in these storms, especially ships approaching them on their equatorial sides; but at the same time it must be admitted that more than one intelligent seaman, who thought himself well up in the subject, has actually run into the very centre of a cyclone, when, by all known rules, he ought to have been certain of avoiding it. There must be some reason for such an error, and it is that reason that I have been seeking for, and which, I trust, I have to some extent discovered. I send you a diagram on a large scale, which will explain my views more clearly than any written description. I assume that within a diameter of 40, 50, 60, 70, or 80 miles, a true circular storm of terrific violence must be found in every so called hurricane, and that to a considerable distance outside and around this central and circular storm winds are to be found gradually decreasing in force from 11, near the outer edge of the

central storm, to 7 and 6, at the outer edge of the bad weather, but which, instead of blowing in ever enlarging circles farther and farther out from one common centre, are always converging to that centre, and on all sides gradually increasing, until, at a certain distance from the central calm, they acquire the force of a hurricane (12), and thence inwards blow with great violence in what, in all probability, is as nearly as may be a circle. It is these converging lines of wind that are, I think, likely to lead men into error as to the position of the centre of the storm. In the remarks I make I shall, to prevent confusion, confine myself to cyclones south of the equator, every one acquainted with the cyclonic theory knowing that the inverse of rules for the guidance of seamen in the southern hemisphere will be the rules for their guidance in the northern hemisphere. Let us suppose that a ship bound to Europe arrives at the point marked * in the outer converging curve traced on my diagram, the wind being N.E. with force 7, that is, double reefs and jib—barometer falling, sky overcast, confused swell, and, in short, every appearance of bad weather—lat. 12° S., long. 70° E.—What ought her commander to do? 'Heave to on the port tack,' says one, 'and wait for the weather to clear.' 'Run to the S.W.,' says another, 'and make use of the storm.' Being a pushing fellow, he makes up his mind to run, and, truth to say, there are as many reasons for approving that proceeding as for finding fault with it. If he succeeds in making use of the hurricane, he is considered a smart fellow; if he runs into it, and is dismasted or worse, 'rash,' 'headstrong,' 'ignorant,' &c., are the best terms he can look for; and yet he might as easily have been wrong in heaving to as in running. The wind being N.E., he infers that the centre bears N.W. He considers that the barometer and weather indicate that he is on the S.E. edge of a cyclone—the N.E. wind upon which he is running forming part of a circular storm, and that necessarily the centre is N.W. of him. Considering, further, that in that lat. and long. the storm is probably travelling W.S.W., he thinks that if he runs S.W. he will be diverging from it, and, that by making use of the storm he will get fine runs perhaps for days to come. But if the N.E. wind be only converging towards the fearful storm raging near the centre, that centre, in the first place, bears W. by N. ½ N., instead of N.W., so that the vessel, by steering S.W. is not diverging from the centre, as was supposed, but is really drawing nearer to it. In due time the weather gets worse from this very cause; the wind veers more to the eastward, the barometer continues to fall, and the captain begins to doubt whether the storm may not after all be progressing more to the southward than he supposed; whether, indeed, it may not, although so far to the eastward, be actually recurving, and he naturally becomes anxious and uncertain what to do. If he decides on running at all risks, he finds the wind still drawing at first more and more easterly, and then more and more southerly, always increasing in fury, and the sea becoming more and more heavy and tumultuous. But run he must now, and he must run dead before it, and being on what I have supposed a line of wind converging to a centre, he finishes by getting into the real hurricane, and loss and disaster are imminent. He may, however, if his ship be tight and staunch and runs well, get round to the N.W. side of the storm, and so get clear, probably with loss of spars and sail; but he has clearly run into what he was running to avoid, because he was under the impression that winds within the influence of a cyclone, although far from its centre, blew in circles round that centre, the wind everywhere clearly indicating the exact, or nearly exact, position of that centre. These opinions I submit with very great diffidence for the consideration of seamen and cyclonists. I am not going to attempt the setting up of any dogmatic theory of my own, but I am inclined to think this theory of converging winds will probably account for the manner in which many vessels have become entangled in hurricanes when seeking to avoid them according to cyclonic rules. Like all other theories on this very important subject, it requires very careful consideration; but there can be no possible risk in deducing from it the rule that vessels on approaching what the barometer, the state of the weather, and the force of the wind, clearly indicate as the dangerous side of a cyclone, should, in seeking to avoid it, keep the wind quite four points on the port quarter. With the wind thus free, a fast ship would run with great rapidity through the water, and, unless the storm were advancing on her in a direct line, would be always increasing her distance from its centre, and getting into finer weather, and, in any case, would have a very good chance of running across its track and thus avoiding it. Ships running into cyclones on their equatorial sides are to a very great extent without excuse. There are, however, some exceptional instances; but they are very rare." The chairman, in thanking Captain Wales for his interesting and valuable communication, expressed the hope that the important suggestions it contained would be taken advantage of by seamen, and prove to be serviceable to them in their attempts to avoid the dangerous parts of cyclones. The diagram prepared by Captain Wales fully explained how it might happen that a vessel, by seeking to keep away from the centre of what was considered a circular storm, would be actually running into it. The secretary was glad that the subject had been taken up by a sailor of long experience and of great practical knowledge and skill, and he had no doubt that Captain Wales's remarks would receive the serious attention they merited. In various papers published during the last fifteen years, he (the secretary) had often called attention to the incurving of the wind in cyclones, and to the losses occasioned by acting upon the supposition that the bearing of the centre was at right angles to the direction of the wind; and he believed that it was now beginning to be admitted that the movement of the air in a cyclone was not at all represented by concentric circles, but by a figure similar to that sketched by Captain Wales. The description given by Captain Wales of the way in which vessels might get involved in a cyclone, whilst acting according to accepted rules, applied to many cases which actually occurred. Captain Wales had framed a practical rule based upon observed facts, and it was for seamen to test its value.

PARIS

Academie des Inscriptions et Belles Lettres, July 14. Two seats of *associés libres*, vacant by the death of MM. Prosper Merimée and Deheque, have been filled at the recent sittings. M. Merimée's seat was given to M. de Robert, and M. Deheque's to M. Thomas Henry Martin, director of the Academy at Rennes. This gentleman has written many valuable volumes on interesting points of history; among others, "On the Physical Opinions of the Greeks and Romans." He was one of the few French savants opposed to M. Chasles' famous letters of Newton, and has written a pamphlet on the subject.

Academie des Sciences, July 17.—M. Faye in the chair. A committee was appointed to discuss the respective merits of several candidates for a free associate membership. The committee was composed of MM. Combes and Bertrand for the section of mathematical sciences, MM. Chevreul and Boussingault for the section of physical sciences, MM. Raulin and Bussy for the free members. The chairman of the committee is *de jure* M. Faye. When a report is to be drawn on the respective merits of ordinary members, the committee is composed from the section to which the late member belonged in his lifetime. In the secret committee held after the public sitting, a discussion was raised as to several candidatures, and it was impossible to come to any definite conclusion.—M. Lacaze Duthiers, a professor at the museum, who claims a seat in the section of zoology, read a paper on a new organ of nervous power which he has discovered in certain gasteropods living in water. This organ is placed behind the œsophagus, and at all events its dimensions are very small indeed. The Academy has appointed a committee to report on the prize Bordin, which is to be awarded this year for the best paper on the function of the stomata in the leaves of plants.—On the 3rd of October, 1870, M. Egger proposed the translation of the four books on Optics by Ptolemy, which were translated from Arabic into Latin, and of which two copies exist amongst the MSS. in the National Library. This suggestion was not lost, as the Royal Academy of Turin passed a resolution to raise the funds required for its publication. Other copies of the same Latin translation are also to be found in the Ambrose Library at Milan, and will be used for the purpose. The translation is very difficult, having been unsuccessfully attempted once in Italian, and once in French.—M. Leverrier presented a report on the observation of falling stars, for August 1869. The phenomenon was observed in twenty-seven different stations, viz. Agde, Barcelona, Bordeaux, Chartres, Chebli (Algiers), Genoa, Grenoble, Le Guerche (Cher), Larenore (Basses Pyrenées), Le Mans, Lyons, Marseilles, Mer (Loire et Cher), Metz, Moncalieri, Montpelier, Nice, Orange, Perpignan, Rochefort, Sainte Honorine (Calvados), Toulon, Toulouse, Tremont, Turin, Valencia. Observations were made by competent observers with correct chronometers, and special maps prepared by the Association Scientifique de

France, of which M. Leverrier is now the chairman. The discussion on the observations is a long work which is not yet finished in consequence of the late war. The observations could not be completed in 1870, but the Association Scientifique de France is resuming its labours, and will be ready to make observations by August 1871 on the former principles.—M. Leverrier sent the description of a bolide observed at 10h. 6m. in the afternoon, 1° 30′ higher than σ Andromedæ, and exploding in Pegasus. He asks for some observations from the astronomical public.—The same question is put as to a magnificent falling star seven times larger than Jupiter observed by M. Chapelas 11h. 12m. in the afternoon, on the 18th July, from ξ Pegasus to the horizon in the north-west. It must have been seen in England.—At the last sitting we omitted to mention the presentation of some grains of wheat, &c., burned by electricity in a storm, a few years ago and preserved as a great curiosity.—M. Bert, Professor of Physiology at the Museum, formerly a prefect of Lille during the latter part of the war, sent a most interesting paper on the influence that the diminution of pressure exerts on animal life. Living frogs were placed under the air-pump, and proved to be killed very soon if pressure is diminished quickly to seven or eight inches, but if diminished gradually, they can live in a more perfect vacuum if proper precautions are taken to renew the residual air offered to them for respiration. Certainly the same thing can be said of aëronauts, who cannot reach a high level without inconvenience, except by very gradual ascent.—M. Dumas presented a small pamphlet from M. Janssen, narrating his ascent on December 2 with Volta. Dr. Janssen was himself the aëronaut, and his ascent was the occasion of some interesting observations. He was appointed a commissioner for visiting the meteorological establishments in England, and reporting upon them, and is now on his way to London.—M. Beaugrand, an engineer in the Parisian hydraulic service, presented a report on Roman aqueducts. He has written a very long essay on the matter, which would have been burned by the Communists with his office at the Hotel de Ville, if he had not brought it home on purpose to write out of it a paper for the Academy.

Vienna

Imperial Academy of Sciences, April 13.—Prof. von Reuss reported on the fossil remains of a crab found in the Leithakalk of the Rauchstallbrunn pit near Baden. The fossil most nearly approaches the living genera *Actæon* and *Daira*.—Prof. A. von Waltershofen reported on a new thermopile of great efficacy.—Prof. V. Graber communicated a memoir on the physiology and minute anatomy of insects, especially the Pediculina, in which he treated chiefly of the Malpighian vessels and tracheæ. The former in many cases consist merely of prolongations of the peritoneal membrane.—Prof. V. von Lang presented a memoir containing researches upon the influx of gases, undertaken for the purpose of testing the laws which have been established for the dependency of inflowing gases upon the pressure.—Prof. C. von Ettingshausen presented a first memoir upon the flora of Sagor in Carniolia, in which he described numerous species of fossil plants from the brown coal of that locality. This memoir included the Thallophytes, vascular Cryptogams, Gymnosperms, Monocotyledons, and Apetalæ. The Thallophytes include a *Sphæria* nearly allied to the Greenland species, and a *Laurencia*, which is the only marine plant found in the deposit. Of the Coniferæ *Glyptostrobus europæus* and *Sequoia Couttsiæ* are the most abundant, and of the latter genus three other species occur. A *Cunninghamia*, very like *C. sinensis*, is remarkable as adding a new genus to the Tertiary flora. Grasses are rare, but Naiadæ are abundant and remarkable. A *Pandanus* and a species of palm occur. Among the Apetalæ the author noticed two species of *Casuarina*, one of which is new and allied to *C. quadrivalvis*. The other orders represented are Myricaceæ 3 species, Betulaceæ 6, Cupuliferæ 15, Ulmaceæ 4, Celtideæ 2, Artocarpeæ 2, Salicineæ 2, Nyctagineæ 1, Monimiaceæ 1, Santalaceæ 4, Daphnoideæ 2, Proteaceæ, 21, Moreæ 19, and Laurineæ 18.—Prof. Carl Koritska exhibited and explained a hypsometrical map of the Alban Mountains, with profiles and views. He regarded the district as particularly instructive, from the intimate collocation of the three forms of volcanic craters and their apparent transition one into the other which prevails there.—Dr. E. Klein communicated a contribution to the knowledge of the Malpighian corpuscles in the human kidney, by Dr. Victor Seng; and a contribution to the knowledge of the finer nerves of the buccal mucous membrane, by Dr. E. Elin.—Prof. Ludwig Boltzmann presented a memoir containing several propositions on the equilibrium of heat, and another on the main proposition of the mechanical theory of heat.—Prof. E. Weiss furnished the elements and ephemeris of the comet discovered by Winnecke at Carlsruhe on the 7th April.

April 20.—Prof. C. von Ettingshausen presented a memoir on the leaf-skeleton of the Loranthaceæ.—Prof. Simony noticed some peculiarities of the glaciers of the Dachsteingebirge. The Gosau glacier descends to an elevation of 6030 feet, the Hallstatt glacier to 6115 feet, and the Schladminger Ferner to 6935 feet. The most instructive moraine phenomena are presented by the lower part of the Hallstatt glacier.—Prof. Seeger presented a memoir on the methods at present employed for detecting small quantities of sugar in the urine, which he regards as unsatisfactory.—A paper on the perforations in the vessels of plants, by Dr. Tangl, was communicated by Prof. Ad. Weiss.

April 27.—Prof. Lang communicated some remarks on the abnormal dispersion observed by Christiansen and Kundt in solutions of fuchsine, cyanine, &c. He showed that the appearance is due to the defective achromatism of the human eye.—M. F. Schwackhöfer reported on the occurrence and mode of formation of phosphorite balls in Russian Podolia. He stated that these balls were originally carbonate of lime formed by concretion, and converted into phosphate of lime formed by the lixiviation of the Silurian clay slate in which they occur, which contains phosphoric acid. The analysis of these balls led to the formula $3\,(Ca^3\,P^2\,O^8) + Ca\,Fl^2$, agreeing with that of apatite in the proportion of fluorine.

Philadelphia

American Philosophical Society, April 21.—Dr. Geuth described the results of recent investigations by himself into Corundun pseudomorphs of Hersinite, an aluminate of oxide of iron, from specimens from Bengal. He reported finding specimens of Hersinite in N. Carolina Corunduns, and believes the emery of Massachusetts is to be referred to the same mineral. In Chester County, Penna., "Corundun pseudomorphs" occur which are quite soft like talc or scaly talc, which prove to be Margarite. A third pseudomorph very much foliated has not yet been determined.—Prof. Cope presented a paper entitled, "A preliminary report on the Vertebra discovered in the Port Kennedy Cave."—Prof. Cresson stated that the young and tender shoots of the *Symplocarpus fortidus* (skunk cabbage) had forced themselves through a solid asphaltum composition pavement two inches in thickness in many places in "Belmont Glen," Park. The road was used for heavy hauling at the time.—Prof. J. P. Lesley described a discovery which he had made in East Tennessee of a sharp anticlinal axis crossing the coal measures of the Cumberland Mountains at right angles to the dominant system of disturbances, and showed its important bearing on the question of the conversion of the northern anticlinals of the Alleghanies, into the southern system of downthrows. Also its relationship to the latter and to the cross undulations worked out by Joseph Lesley in his instrumental survey of E. Kentucky thirteen years ago; and to the N.W.-S.E. system of faults described by Owen, Hall, and other geologists in the Valley of the Mississippi.

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